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THE PSYCHOLOGICAL REVIEW

HULL'S SYSTEM OF BEHAVIOR: AN EVALUATION

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The aim of this article is to present briefly the essential characteristics of Hull's system, past and present, and to evaluate it from the standpoint of scientific method. No attempt will be made to compare it with other approaches or to weigh its possible applications to individual or social welfare. The logic of science provides a sufficient criterion for our purpose.

In his presidential address to the American Psychological Association in 1936 Hull stated the requirements of a sound scientific theory as follows:

> 1. A satisfactory scientific theory should begin with a set of explicitly stated postulates accompanied by specific or "operational" definitions of the critical terms employed.
>
> 2. From these postulates there should be deduced by the most rigorous logic possible under the circumstances, a series of interlocking theorems covering the major concrete phenomena of the field in question.
>
> 3. The statements in the theorems should agree in detail with the observationally known facts of the discipline under consideration. If the theorems agree with the observed facts, the system is probably true; if they disagree, the system is false. If it is impossible to tell whether the theorems of a system agree with the facts or not, the system is neither true nor false; scientifically considered, it is meaningless (**38**, p. 5).

The proposed examination of a specific theory should help to make these standards more concrete and to point up their implications for a systematic experimental psychology.

METHODOLOGY

Role of Deduction

Hull's version of scientific method is sometimes described as deductive, geometric, or postulational. These terms imply a hierarchical structure in which the basic principles depend on the experimental testing of deduced theorems for proof. Actually, Hull became dissatisfied with this method in its extreme form and in 1938, by his own account, he abandoned it "for that of ordinary mathematics. This of course required the formulation of the postulates in such a way that they could be converted into equations" (**53**, p. 157). As a result, Skinner (**78**) accused Hull of inconsistency for using the results of experimental measurement as postulates in a supposedly deductive system. There are two answers to this criticism:

1. There seems to be no logical requirement that a postulate must be inaccessible in order to generate deductions. The axioms and postulates of Euclidean geometry are not especially obscure. Hull (**38**) distinguished two kinds of postulates: those open to direct observation and those that can be only indirectly confirmed. Surely the former are more likely to inspire confidence. All Skinner could have meant is that indirect verification of an empirically determined postulate is superfluous. But it will be a long time before all postu-

lates are known laws, and meanwhile few experiments can be designed to test just one such law, free of all untested assumptions. Until that time comes, the deductive process is still necessary.

Hull's position in this respect is quite orthodox. It stands on the assumption that complex behavior is the product of many interacting processes and that psychology's business is to analyze it into its determiners. These can best be revealed in simple situations, preferably one at a time. When enough basic principles—or postulates—have been isolated, it may be possible to deduce more complicated bits of behavior. The assumption may be false, but the logic is unassailable.

2. In spite of Hull's deliberate attempt to fit his postulates to empirical data wherever possible, we find just one postulate, and that only in part, that states a direct relation between stimulus and response variables.[1] All the others that are based on actual measurement are derived therefrom by one or more substitutions in the original equations. In so doing, incidentally, Hull shows little concern for the primacy of his postulates. The equations found in Postulate II,[2] for example, were derived with the aid of XII, a variant of VIII, and VI. Postulate VI, in turn, was arrived at by way of the relation stated in XIV. Status in the hierarchy seems to be determined more by the probable importance of a principle than by its origin. But the point is that most of the postulates are one or more steps removed from mere curve-fitting. This brings us to a second major feature of the system.

[1] Postulate IX E (**52**) gives the number of unreinforced responses to massed extinction as a function of the work involved in the response.

[2] Hereafter upper-case Roman numerals will refer to postulates, lower-case Roman numerals to corollaries, as numbered in *A Behavior System* (**52**).

Use of Intervening Variables

In their latest form the postulates and corollaries, with the exception noted above, can be divided into four categories: (*a*) Qualitative or quantitative statements about unobservable processes, with general implications for behavior, e.g., I, III, XI, XII. (*b*) Quantitative statements of relation between some environmental change and an intervening variable, e.g., IV, V, VI, VII. (*c*) Quantitative statements of relation between some response variable and one intervening variable, i.e., reaction potential (${}_sE_R$), e.g., XIII, XIV, XV, XVI. (*d*) Rules of integration of intervening variables, e.g., VIII, iv, vi, viii.

In view of their prominent role, it is important to examine the intervening variables more closely. Are they actually "hypothetical constructs" in the sense that they owe certain properties to observations at a different level, e.g., physiological or introspective (**64**, **79**)? A studied answer must, I believe, be no. It is true that a few concepts, such as *afferent stimulus interaction, behavioral oscillation,* and the *reaction threshold,* smack faintly of neurology. Closer inspection shows that these are concepts not yet tied down at the stimulus end. They are inferred, however, solely from observations of behavior. Even the most likely suspect, the stimulus trace, is defined as the logarithm of its *molar stimulus equivalent* (S'). In turn, S' can be understood only in terms of its derivation; its values come from the equation for *stimulus intensity dynamism* (V), which is itself derived from the variation of reaction time with *external* stimulus intensity.

As this example indicates, Hull's postulates turn out to be largely elaborations of the basic formula, Response = f(Stimulation). What function, then, do these "unhypothetical constructs" serve? Students of theory construction

(63, 82) have considered this question in detail. Briefly, any single empirical equation of wide applicability would necessarily include many parameters of unknown significance (88). Hull (43) followed Tolman (87) in contending that such an equation would be more useful if it were broken down into parts, each part representing a function of a measurable change either in the environment or in the organism's response. By entering different values of these functions, behavioral results could be predicted; by arranging the corresponding conditions in a controlled experiment, the prediction could be verified.

It would be a mistake to consider this happy state of affairs a *fait accompli;* rather, as Hull (43) stated plainly, it is a goal to be achieved only by laborious trial and error. A glance at the historical development of the system reveals definite progress in that direction.

HISTORY

In his autobiographical chapter (53) and in connection with his revised postulates (49, 51), Hull outlined briefly the growth of his theory. Although an exhaustive treatment would include his seminar memoranda and, ideally, his personal notebooks, most readers are limited to his four theoretical books (42, 51, 52, 55) and to the series of articles in the *Psychological Review* and other journals from 1929 to 1950. The present sketch will be confined to these published sources.

Hull's first theoretical paper, "A Functional Interpretation of the Conditioned Reflex" (28), set the stage for the entire project. In its opening paragraph he stated the theme which was to dominate all his efforts when he described the conditioned reflex as "an automatic trial-and-error mechanism which mediates, blindly but beautifully, the adjustment of the organism to a complex environment" (28, p. 498). The same theme motivated him to entitle his presidential address, "Mind, Mechanism, and Adaptive Behavior" (38), to devise "mechanical brains" (5, 58), and to open the last chapter of *A Behavior System* (52) with a summary of eight "automatic adaptive behavior mechanisms." His first article posed problems that we find constantly recurring: generalization and discrimination (41, 46, 50), patterning (44), short-circuiting (30, 32), and the "dilemma of the conditioned defense reaction" (see 6, 67, 72).

It is interesting to find in 1930 a study of simple trial-and-error learning (29), since one of Hull's theoretical achievements was to derive both Pavlovian and Thorndikian learning types from a single set of assumptions (38). Two constructs assumed central importance in this period, the response-produced cue and the goal gradient. Twenty years were to elapse before these concepts were brought together in a single derivation. Meanwhile they enabled Hull in a series of papers to unify an impressive array of problems: the integration of serial responses (30, 31), spatial orientation (33), "reasoning" (36), and "field forces" (39).

From time to time more ambitious ventures in the form of "miniature systems" appeared (38, 41). One, an application of conditioning principles to rote learning, provided what Hull called a "dress rehearsal" for the main event (53). It started in 1931 with Lepley's hypothesis linking remote associations with trace conditioned responses. A preview in 1935 (37) was followed five years later by the publication of the *Mathematico-deductive Theory of Rote Learning* (55). In some respects this monograph proved to be the most remarkable accomplishment in the history of psychology. As a cooperative undertaking of mathematicians, logicians, and

psychologists, it is probably unique. By our adopted criteria—clear and consistent postulates, rigorous deductions dovetailing with experiments—it is unsurpassed. Unfortunately it suffers, in my opinion, by being ahead of its time. So few of us are educated to this sort of presentation that we are likely to keep a respectful distance. Even experimenters in rote memory tiptoe around it and go on with their researches. This is all the more deplorable since, as Hull intimated (53), in the light of his analysis of compound trial-and-error learning the theory is already due for a complete remodeling along the lines proposed by Eleanor Gibson (13). Its most useful function may well be to serve as a blueprint for the designing of miniature systems in the future.

Principles of Behavior (42) presented what proved to be a preliminary draft of the basic postulates of the system. Already Hull had moved away from qualitative generalizations to more analytic statements. But by the time he was ready to publish a set of deductions for individual behavior, he had moved so much farther that a revised edition was necessary. Even the revision would not stand still. Between its publication (49, 51) and its reappearance as the introduction to *A Behavior System* one major change, the derivation of ${}_{S}E_{R}$ as a function of delay of reinforcement (J), was already evident.

Intimately related to this "growth potential" is the interdependence of theory and experiment that makes itself felt throughout Hull's publications. Both aspects of this relationship are equally clear. On the empirical side, a series of experiments on simple trial-and-error learning (40), the goal gradient (34), and rote learning (35) paralleled his early papers. As the system took shape, experiments from his laboratory served one of two functions: to provide equations for the postulates or to verify deductions. A few outstanding examples are the following: Hovland's two series on generalization (21, 22, 23, 24) and rote learning (e.g., 25, 26, 27); Perin's work on delay of reinforcement (70, 71) and the joint determination of resistance to extinction (69); the quantification of reaction potential (12, 15); and the series by Arnold and others on behavior chains (1, 2, 3, 4) and compound trial-and-error learning (14, 45, 47, 86).

On the other hand, Hull's constant search for general laws kept his theory always a step—sometimes a long step—ahead of the data. A distinguishing characteristic was his ability to grasp the significance of a specific hypothesis, develop its implications, and bring it into relation with other principles in an integrated whole. His elaboration of Lepley's hypothesis into a system of rote learning is a case in point. From Mowrer's (68) and Miller's (66) fatigue-reduction hypothesis of extinction came the dual construct of reactive and conditioned inhibition by which Hull explained, among other things, disinhibition, reminiscence, and the law of less work (42). His adaptation of Spence's theory of discrimination learning (80, 81) is less an extension than a quantitative refinement (50). But nowhere is his ingenuity and daring better shown than in the use of Thurstone's scaling procedure for the quantification of reaction potential (54). Some critics think such trial flights foolhardy; to others they represent "the priceless ingredient." Hull himself recognized their uncertainty and left the verdict to experiment.

PRESENT STATUS

Readers of *A Behavior System* will not find a finished product; it is obviously a cross section at a stage of development. In it Hull did two things:

(*a*) he presented a further revision of the postulates and corollaries together with the latest derivations, and (*b*) he educed 133 theorems as a sample of the deductions to be yielded by the system. Let us take up these two features in order, applying our criteria where relevant.

Postulates and Corollaries

Here we are chiefly concerned with the changes that distinguish the present set from its forerunners. Only major developments will be considered.

Quantification. Most noticeable is the increased mathematical precision in the form of presentation. For the first time constants are designated by numbers instead of letters, and are often carried to several more places than are significant. Hull's treatment of the stimulus trace is an extreme example. In 1943 he described it as an afferent impulse rising quickly to a maximum and "gradually diminishing to zero as a simple decay function of its value at the time the stimulus energy ceases to act" (**42**, p. 47). In 1952 he defined it in terms of its molar stimulus equivalent ($\dot{S}'$) which rises and falls as a power function of time (t) substantially according to the following equations (**52**, p. 5):

$$\dot{S}' = 465{,}190 \times t'^{7.6936} + 1.0,$$

$$\dot{S}' = 6.931(t' + .01)^{-1.0796},$$

where $t' = t - 0.45$ sec.

Closely related to this property is the restricted range to which the postulates apply. Drive (D), formerly "proposed as a common denominator of all primary motivations" (**42**, p. 239), is now defined only for food privation (VA, B). Incentive motivation (K) is defined by the equation

$$K = 1 - 10^{-a\sqrt{w}},$$

in which w is the weight of food given as reinforcement (VII). Reaction potential has become "an increasing linear function of the Tarchanoff galvanic skin reaction amplitude" (XV). To be convinced, one need only look up comparable postulates in the earlier set.

Applying criterion 1—that postulates should be stated explicitly, with operational definitions of the critical terms—we must conclude that Hull kept this ideal in mind and approached it more and more closely. One consequence, however, should not be overlooked. It is clearly demonstrated that the so-called postulates are actually inductive generalizations, to be verified for a variety of specific conditions. As data accumulated, Hull was able to define his constructs in more strictly operational terms, and gain in precision what he lost in scope. In a sense he was following the statistical practice of presenting a sample value as the best estimate of the "true" one. But the analogy holds good only on the assumption that, as different drives, incentives, external conditions, and responses are explored, certain uniform relationships will emerge with constants differing only by chance. A test of the validity of that assumption deserves high priority.

Motivation and learning. A more refined analysis of the interaction of learning and motivation shows itself in several ways. In the first place, some noteworthy changes have taken place in the constitution of ${}_sE_R$:

1. Functions originally included in habit strength (${}_sH_R$) now enter ${}_sE_R$ directly. In 1943, Postulate 4 and Corollary II (**42**, pp. 178, 253 f.) defined these two constructs roughly as follows:

$${}_sH_R = M \times K \times J \times T \times f(N),$$

$${}_sE_R = f(D \times {}_sH_R),$$

where M equals 100 *habs*, N is number of reinforcements, J is a function of delay of reinforcement, and T is a function of the time between stimulus and response. K and J, as already

noted, are functions of amount and delay of reinforcement, respectively. In 1952, in Postulates IV and VIII (**52**, pp. 6, 7), the corresponding expressions are:

$$_{S}H_{R} = f(N),$$
$$_{S}E_{R} = D \times V \times K \times {}_{S}H_{R},$$

where V, as noted earlier, stands for stimulus intensity dynamism (**48**).

Referring to this reorganization (**51**, p. 122), Hull called the earlier statement unclear owing to expository difficulties arising from the order of presentation. This implies that there is no "real" difference between the two. Again, in connection with the delay-of-reinforcement gradient (**52**, p. 133), he suggested that, since $_{S}H_{R}$ can never be observed apart from $_{S}E_{R}$, we may be dealing with a pseudo problem; i.e., one with no technique for its solution. A method has long been in use, however, for settling this question. It consists of training subjects to an asymptote under one value of, say, K, then shifting them to another. If their response to the change is immediate and complete, as compared with control groups, the factor belongs exclusively to $_{S}E_{R}$; if it is slow, to $_{S}H_{R}$. It must have been with such experiments in mind that Hull qualified Postulate VIII by the clause, "where conditions are constant throughout learning and response-evocation." But when he discussed Crespi's results on varying K he sidestepped the issue, in my opinion, by adding a new postulate (**52**, p. 142).

2. Comparison of the two pairs of equations given above shows that V has replaced J and T. It would be more accurate to say that with the aid of V Hull was in a position to derive the other two functions indirectly. Essentially he defined V as a function of stimulus intensity, used it to derive S' as a function of time, and made the effect of delay dependent on that function. Hull did not actually deduce T, though in deriving J he showed how it could be done.

But the introduction of V raises some interesting questions. First, there is the operant-respondent dichotomy to consider. Conditioned respondents such as salivation and the galvanic skin response will presumably show a correlation with conditioned stimulus intensity (**22**, **59**). Measures of strength of discriminated operants—strength of pull, number of choices, response latency, voluntary reaction time—have also been found to vary with intensity of the discriminative stimulus (**48**, **57**). But how V would be estimated in a simple bar-pressing situation, if at all, is hard to say. Just when does one measure V and how heavily does one weight it?

A second question concerns the relation between V and learning. This is simply a special case of the problem already considered; i.e., does V "belong" to $_{S}H_{R}$ or $_{S}E_{R}$? Evidence on resistance to extinction as a function of conditioned stimulus intensity during training is uniformly negative (**10**, **16**, **17**). Yet what is true of external stimuli may not hold for traces, or even for their "molar stimulus equivalents." Recent experiments on the effect of change in delay of reward (**18**, **61**, **77**), as well as Perin's earlier data (**51**, pp. 52 ff.), suggest that delay determines not only what an animal does but what it learns. If this is true, Hull may be correct in his treatment of trace conditioning (**52**, pp. 102 ff.) and delayed reward learning (**52**, pp. 128 ff.) when he multiplies $_{S}H_{R}$ by V_1 of the trace before entering it in $_{S}E_{R}$. But Hull himself is uncertain on this point (see **52**, p. 131, footnote 2). And although he includes the same provision in Postulate X B on stimulus generalization, he abandons it

in working out illustrative examples (**52**, pp. 79 ff.).

A second development in the treatment of learning and motivation is the renewed emphasis on the fractional antedating goal reaction (r_G). After carrying the chief burden of Hull's earlier arguments, this construct was barely mentioned in *Principles of Behavior*, although it continued to play an important part in the thinking of Spence and his students (**62**, **84**, **85**). It now resumes its place in the system in the form of two corollaries. One (xv) makes explicit the secondary reinforcing power of the r_G-produced stimulus (s_G). The other (iii) represents the derivation of the temporal gradient of reinforcement in behavior chaining (A) and simple delay (B). Briefly, Hull supposes that the secondary reinforcement provided by s_G is one of the factors determining ${}_sE_R$. In delayed reward learning s_G is produced by the conditioning of R_G to S', which becomes weaker the longer the delay. Moreover he assumes—and could probably have deduced—that the weaker the conditioned S', the weaker will be the antedating r_G. The falling gradient follows.

Hull's more mathematical derivation, carried through step by step in terms of a numerical example, calls for a detailed analysis which cannot be attempted here. It is enough to point out that he has, in effect, put Spence's (**83**) theory of delayed reward learning in quantitative form and included it as a logical part of his system.

Another example of revived interest in r_G appears in Hull's treatment of extinction. In his earlier writings he had put forward a *frustration hypothesis*, according to which the blocking of an accustomed response was one of two conditions leading to extinction (**33**, p. 139; **38**). But the theory developed in *Principles of Behavior* relied exclusively on the second condition, i.e., response evocation without reinforcement. This theory is now restated in modified form in Postulate IX and its corollaries, dealing with inhibitory potential (I_R and ${}_sI_R$). In addition, however, under the heading, "The Realization of an Anticipation and Its Frustration," Corollary xvii is offered, giving as a condition of extinction "the abrupt cessation of the customary reinforcement of a previously learned act" (**52**, p. 134). Just how this effect is produced is not made clear. It appears that prevention of "the goal behavior formerly evoked by s_G" somehow generates inhibition (I_R), which then becomes conditioned to s_G and presumably counteracts its reinforcing effect. That is, if $s_G E_{r_G}$ is a factor in ${}_sE_R$, extinction should be complete when $s_G E_{r_G} - s_G I_{r_G} = 0$. A detailed development of this hypothesis might explain "nonresponse extinction" (**11**, **76**; but cf. **9**), heretofore more amenable to expectancy theory.

These changes in the constitution of ${}_sE_R$ and in the use of r_G are in line with a trend toward increased emphasis on motivational determinants. In 1937, in connection with his derivation of goal-striving, Hull wrote: "the fundamental phenomenon of motivation seems to have been derived from the ordinary principle of association. . . . If this deduction should prove to be sound, it will have reduced the two basic categories of motivation and learning to one, the latter being primary" (**38**, p. 14, footnote 16). Drive at that time was represented simply as a persisting stimulus (S_D) helping to bring r_G forward to the beginning of a response sequence.

By 1943 primary motivation (D) had been introduced as a factor coordinate with ${}_sH_R$ in determining ${}_sE_R$. But reinforcement, attributed primarily to S_D-reduction, was still held to strengthen ${}_sH_R$ according to the amount and delay of reward.

In his latest book Hull has substituted Spence's notion of immediate secondary reinforcement by a concurrent s_G. He has thus made possible, though not quite explicit, a construct of *goal motivation* ($s_G E_{rG}$) apart from ${}_sH_R$. This idea of a motive originating in the goal response is essentially what I have called *tertiary motivation* (**73**, **74**, **75**) to distinguish it from the secondary motivation of conditioned drives.

Theorems

Considered in historical perspective, the deductions of *A Behavior System* take on added interest. A majority of them concern problems Hull had already tackled in the 1930's; in these cases the earlier and later treatments form a crude scale by which to measure intervening growth. His derivation of the habit-family hierarchy, for example, remains virtually unchanged (cf. **33**); that of simple trial-and-error learning is quite unrecognizable (cf. **29**). My purpose, however, is not to pursue this comparison but to apply to the present deductions our second and third criteria of scientific method.

Logical rigor. With respect to logical articulation, the theorems are, for the most part, in a fairly primitive state. Deductions are informal and elliptical, not to be compared with the tightly knit structure of the rote-learning monograph (**55**). There are several obvious reasons. For one thing, the present work covers such a wide range of problems that it would take an appalling number of man hours to reduce them all to logical order. Then there is the practical problem of communication; a highly technical presentation would naturally restrict its influence. A third reason, quite sufficient by itself, is that not enough is known about most of the areas covered to support a rigorous logic.

For the same reason, paradoxically, we find Hull's theorems less precise, on the whole, than his postulates. The paradox is resolved when we note that a postulate may be drawn from an experiment performed under controlled conditions, while a theorem predicts an outcome in so far as it is determined by, say, two postulates and a corollary, but with other conditions and relationships unknown.

By our second criterion, theorems "should be deduced by the most rigorous logic possible under the circumstances." It is hard to apply so elastic a rule to the deductions as a whole, especially since they vary so widely in quality. This very diversity, however, suggests another approach. We may ask what circumstances determine how rigorous a logic is possible? A tentative answer is that predictions can be more precise the simpler the phenomena and the more is known about them. If, then, the criterion were successfully met, we should expect the various problem areas to show a correlation between these characteristics and the quality of the pertinent theorems. Such a comparison should throw some light on the points of relative strength and weakness of Hull's system as so far developed.

The indicated procedure was to attempt to rate the propositions discussed. For this purpose I equated "logical rigor" with "degree of quantification," which I estimated on a three-point scale. If a theorem stated, explicitly or implicitly, the form of a curve, it received a score of two. If it gave a relationship only in terms of more or less, it was rated one. A mere statement of presence or absence got a rating of zero.

In spite of the obvious defects in this procedure the results for the 136 theorems and corollaries appearing in chapters 2 to 10 are presented in Table 1. Abbreviated chapter headings are listed in column 1, arranged in descending order of mean ratings, as given in column

TABLE 1

FREQUENCY DISTRIBUTIONS OF 136 THEOREMS AND COROLLARIES OF *A Behavior System* GROUPED BY CHAPTERS AND RATED FOR QUANTIFICATION AND EVIDENCE

Chapter	*N*	Quantification				Evidence		
		2	1	0	Mean	2	1	0
6. Behavior chains	9	6	3		1.7	7	2	
3. Discrimination learning	7	4	3		1.6	5	2	
2. Simple trial and error	11	6	3	2	1.4	4	4	3
8. Behavior in space	50	15	26	9	1.1	12	19	19
5. Antedating goal reactions	8	4		4	1.0	7	1	
4. Molar stimulus trace	9	2	4	3	0.9	6	2	1
9. Maze learning	20		13	7	0.6	18		2
7. Individual behavior link	12		7	5	0.6	4	3	5
10. Problem solving	10		5	5	0.5	1	3	6
All chapters	136	37	64	35	1.0	64	36	36

6. It should be noted that where a theorem consisted of several parts they were combined and rated as a unit. Column 2, therefore, does not give the total number of propositions, but rather the number of whole theorems.

In terms of means the nine topics fall readily into three groups of three. Two of the first group, discrimination learning and simple trial and error, deal with the selection of single responses or stimuli under restricted conditions that have been intensively investigated. The third, behavior chaining, is more complex but can be broken down into simpler units and has itself been thoroughly studied in the laboratory. By contrast, all three topics in the last group are highly complex, and two of them—problem solving and acquiring fine motor skills—are hardly accessible to direct observation. In the middle group the theorems on behavior in space cover fairly simple situations, but little research has been done on them. On the whole, the analysis confirms our expectation: the more an area is subject to —and subjected to—experimentation, the more definitive are the theorems in regard to it.

Empirical validation. In conformity with the requirement that a sound scientific theory must agree with the facts, Hull accompanied almost every deduction with a statement about the relevant evidence. When he summarized the statistics on this point, he was able to report that out of 123 propositions on which evidence was available, only one was definitely refuted.[3]

Impressive as these figures are, there are two reasons why mere percentage of agreement should not be weighted too heavily:

1. In their present semiquantitative form many theorems need to be only loosely related to data to be confirmed.

2. A large number of theorems probably owe their existence to the experiments that validate them. Good examples are to be found in the chapter on the fractional antedating goal reaction. There is no logical objection to the practice of attempting theoretical deductions of known experimental results. This is especially clear when a given finding merely determines which theorem one will attempt to derive out of a number of possibilities, all equally derivable

[3] Even this discrepancy vanishes when an error in the deduction is corrected, as I hope to show elsewhere.

from the postulates. It often happens, however, that knowledge of a certain experiment enters, directly or indirectly, into the formulation of the postulates themselves. Again there is nothing vicious about this circularity. An argument *ad hoc,* since it is essentially inductive, simply stops short of a "pure" deduction. Experiments that are used to establish a generalization can add nothing more; they may be called "proof" but not "further proof."

Hull's repeated emphasis on the importance of untried predictions as a test of theory is therefore well taken. He himself put 55 of his propositions in this category.[4] To permit a topic-by-topic comparison I rated the same corollaries and theorems as before with respect to available evidence. Following Hull's lead, I distinguished three categories: direct experiment or conclusive observation; indirect, partial, or suggestive experiment or observation; no evidence. These I rated 2, 1, and 0, respectively. The results appear in the three right-hand columns of Table 1, which show the frequencies falling in each category. It was possible to treat subdivided theorems as wholes with little ambiguity. Since the number of confirmed propositions may mean much or little, I have not calculated mean ratings. Interest centers mainly in the last two columns. These show the problem areas where Hull has pointed clearly to the need and opportunity for experimental attack.

Evaluation. In *A Behavior System* Hull has provided material for an appraisal of the deductive fertility of his system. The picture, as we have seen, is one of asymmetrical growth, well advanced in some directions, embryonic in others. It invites us to consider both what has already been done and what is promised for the future.

The outstanding deductive achievement of the system to date, second only, in my opinion, to the *Mathematico-deductive Theory of Rote Learning,* is Hull's analysis of simple behavior chains. He chose for his empirical model a sequence of four problem boxes, each calling for a single response separately mastered by previous training. The experimental task was to integrate the four responses. Here was a situation simple enough to permit identification and control of the more important variables, complex enough to test the joint action of several principles at once. Moreover, by using either the same response or different responses at the four choice points, and by varying the place of reward, it was possible to demonstrate the application of the theory to four basic combinations.

Hull's feat was to set up a hypothetical numerical example and predict the relative latencies of the four responses. To do so he approximated the gradient of reinforcement (iii A) and the antedating and perseverating stimulus-generalization gradients presumably operating within the chain, summated the expected ${}_sE_R$'s (v, vii) at each response point, and converted the totals to latencies (XIV). A series of meticulous experiments by Arnold (**1**, **2**, **3**, **4**) provided a check on the accuracy of prediction.

It is true that the foregoing procedure fell short of "pure" deduction. Instead of deriving generalization gradients by computing values of S' and their afferent interaction, Hull was forced to resort to simple decay functions with arbitrary constants. But even granting him that much latitude, I find the closeness of fit to Arnold's data remarkable.

What makes this demonstration the more impressive is its generalization to other conditions. Nearest at hand was a parallel series of experiments from the Yale laboratory on *compound trial-and-*

[4] Hull counted parts of theorems separately.

error learning (14, 45, 47, 86), consisting of the integration of four multiple-choice tasks. By converting ${}_{S}E_{R}$ into probability of error, Hull was able to predict the obtained results with surprising accuracy, even in assigning proportions of antedating and perseverative errors to specific choice points. Next came his own findings on the speed-of-locomotion gradient (34), which might be considered a form of homogeneous response chain with terminal reinforcement, and which showed some resemblance to Arnold's data. Finally we note the striking correspondence between *heterogeneous compound trial-and-error learning with serial reinforcement* and rote memorizing of nonsense syllables, both in task structure and in form of serial position error curve. As mentioned earlier, the parallelism was close enough to suggest a thorough revision of rote-learning theory. Although Hull was unable to complete the task, this projected unification of two miniature systems was in itself a proposal of major significance.

For the immediate future the greatest impetus to growth will probably be found in the chapter on behavior in relation to objects in space. A glance at Table 1 suggests why. Although two thirds of the 50 theorems are not more than semiquantitative, three fourths are without substantial confirmation. Such a situation is not likely to go long unchanged.

Most of these theorems deal with behavior in response to two *adient* or two *abient* [5] objects, or to one adient and one abient object, in different spatial arrangements. Although the analogy with Lewin's three types of conflict is obvious, closer inspection throws an interesting light on Hull's position. Just as Lewin (60) drew a sharp line between physical and psychological space, so Hull insisted that "molar behavior theory is not molar physics" (52, p. 243), and refused on that ground to summate behavioral vectors as if they were physical ones. At the same time, restricting his analysis to purely spatial problems, he held Lewin's nonspatial conflicts equally irrelevant. Both assertions can be turned into challenging hypotheses. The physical vector analogy may be risky but it is definitely worth a try. As to the nonspatial analogy, it is by no means farfetched. Hull derived adience and abience from stimulus generalization based on distance cues, but admitted that the relation between distance and "just noticeable differences" (j.n.d.'s) on the stimulus continuum was unknown. Distance itself, in other words, was only an indirect determinant. If Lewin's nonspatial variables (such as the change of expression from a frown to a smile) could be expressed in j.n.d.'s, might not the same principles apply?

It is strange that an area so basic to the understanding of motivation has failed to attract experimenters, especially since the pioneer work of Miller and Brown (7, 8, 56, 65) developed an appropriate technique. Perhaps what this almost virgin soil needs is the kind of preliminary theoretical spadework Hull has done.

A LOOK AHEAD

Unfinished business is the last and most urgent item on our agenda. It would be pointless to attempt a list of unsolved problems. A system as closely tied to experimental research as Hull's offers far too many, and, furthermore, every reader prefers to discover his own. Instead I shall underline what seems to me its greatest weakness and its most promising remedy.

If a building is no more solid than its

[5] It is to be hoped that Hull's adoption of Holt's (20) convenient terms for *approach-inducing* and *withdrawal-inducing* will bring them into the general use they deserve.

foundations, neither is a system more stable than its postulates. That Hull's postulates are far from "finished" is obvious from their rate of revision during the past few years. He is said to have remarked, "I reserve the right to change my postulates every morning before breakfast." There is no reason why we should regard them with greater awe.

In laying his foundations Hull used the best materials available from a variety of sources. While one postulate (II) refers to eyelid conditioning and another (X) to the conditioned GSR, still others are drawn from bar-pressing (IV) or locomotor responses (VII) of rats. It is as if an automobile engine were assembled from parts belonging to different makes of cars. The question is, how well do they go together?

A case in point is the derivation of Postulate VI. To arrive at an expression for V, Hull had to assume that ${}_{S}E_{R}$ bears the same functional relation to bar-pressing latencies in rats as to visual reaction times in humans. Equally doubtful assumptions must be made in attempting to combine two or more postulates in a joint deduction. Nor can we be sure if the constants alone need adjustment or the functions do also. In Postulate XVI, for example, which states the relation between ${}_{S}E_{R}$ and total number of extinction trials, the "same" function is negatively accelerated for massed training trials and positively for spaced.[6]

What is to be done? A few years ago Hilgard (19) made a strong case for miniature systems. I cannot improve his argument, and I find that a study of Hull's behavior theory makes it more convincing. What is needed is an intensive application of Hull's method to more circumscribed areas. Only when the interrelations within each one have been thoroughly explored can we identify common features with confidence. When that time comes, another Hull may be there to put the pieces together.

Models are already available and promising starts have been made. Hull's theories of rote learning and behavior chaining were mentioned earlier. An approach to a miniature system is the group of postulates (IV; V A, B; IX D; XII; XIV; XVI) derived through the use of Thurstone's scaling method and based, with one exception (IX D), on bar-pressing latencies. Other tempting foci are the rapidly growing bodies of data on rate of response in the Skinner box, on runway latencies, on choice-point behavior, and on discrimination learning with joint stimulus presentation. If my prediction is correct, it will not be long before accumulating data on adient-abient behavior will call for integration.

To settle the most pressing systematic issues Hull outlined an experimental program of staggering proportions. Realizing that he could direct only a relatively small part of it, he was anxious to stimulate others to undertake the task. At one time he remarked that he had discovered the best way to do this. Few students or colleagues had ever bothered to do the experiments he suggested. But all he had to do was to make a clear, unqualified statement on a point of theory, and half a dozen experimenters would run to their laboratories to prove him wrong. It was this willingness to be wrong rather than noncommittal, to submit every dispute to the arbitration of experiment (with his favorite wager of a chocolate malted on the outcome!), that permeated all of Hull's scientific work. High on any list of his contributions to psychology

[6] It should be added that Hull used such indirect determinations as heuristic devices only and was careful to point out their makeshift character.

should go the many challenging targets he gave us to shoot at.

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INTRALIST SIMILARITY IN VERBAL LEARNING AND RETENTION

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We have reported elsewhere a series of experiments in which intralist similarity was varied for verbal materials with retention being measured after 24 hours (7, 8, 9, 10). These experiments included both nonsense syllables and adjectives, and both paired-associate and serial lists. In addition, intralist similarity was manipulated independently among stimuli and among responses of the paired-associate lists. Methods used to vary, hence define, similarity are given in the original reports, and no other definition will be given here. Data from these experiments have shown that, over a wide range of response strengths, *recall and intralist similarity are not* systematically *related*. Items in a list with high similarity among these items are as well recalled as are items from a list in which the similarity is low. On the other hand, retention as measured by relearning demonstrates that lists with high intralist similarity take longer to relearn than do those with low intralist similarity. Descriptively, therefore, the results of these experiments may be summarized by saying that frequency of items lost over the retention interval is equal for all levels of intralist similarity, but the higher the intralist similarity the longer it takes to relearn the items lost.

No previous studies have shown unequivocally any facts which are at odds with the generalizations made in the above paragraph, although no previous investigator has studied the matter extensively. The writer considers the explanatory problem of these results to be important for two reasons. In the first place, Gibson's (2) theory of verbal learning has been very useful in organizing the facts of transfer of training in verbal materials. The basic idea of the theory is that interference is produced by stimulus generalization, these generalizing tendencies varying directly with similarity. According to the theory, generalizing tendencies within a list that has been learned will recover with the passage of time and (by producing interferences) become the major (if not only) cause of forgetting. And, of course, the greater the original generalizing tendencies, the greater will be the forgetting. None of our recall results supports this prediction, although some confirmation of the theory can be found in the relearning scores. If Gibson's postulate concerning recovery of generalization tendencies is inadequate, then we must look elsewhere for a theory of forgetting.

The second reason why the present results pose a problem is a compelling empirical one. If similarity as varied here *within* lists is varied *between* lists (as in a retroactive inhibition paradigm), we find that decrement in recall *is* directly related to similarity. Why does recall as a function of similarity between lists conform to theoretical expectations, whereas similarity within a list does not?

The General Hypothesis

The general hypothesis to be presented holds that Gibson's idea that for-

[1] The experimental work reported in this article was done under Contract N7onr–45008, Project NR 154–057, between Northwestern University and the Office of Naval Research.

getting is largely produced by interference resulting from generalizing tendencies is correct, but that its elaboration requires a considerable broadening of our conception of sources of interference.

At the time of recall of any given list we may identify three sources of interference: (*a*) from within the list; (*b*) from previously learned lists; and (*c*) from previously and subsequently learned material which is not a part of the material of the experiment. The present analysis will attempt to show specifically the importance of the first two sources; the importance of the third source can only be argued on the basis of analogy.

In our experiments *S*s served in a series of daily conditions, each condition using materials of a constant level of intralist similarity. Each day *S* recalled and relearned a list learned on the previous day. These successive recalls should be more and more influenced by interference from previously learned lists. This is, in effect, proactive inhibition of retention. It has been shown elsewhere (4) that this increasing interference as a function of stage of practice occurs with paired adjectives of low intralist similarity, and a like finding has been present in all experiments discussed in this paper where intralist similarity was low. If we assume that this decrement in recall is a consequence of generalizing tendencies from previously learned lists, we must then expect that this interlist generalization will be less the greater the intralist similarity. Conversely, this interlist interference will be greater the less the intralist similarity. This will become clear if we examine the structure of nonsense syllable lists as intralist similarity is varied.

Intralist similarity was varied in our experiments with syllables by varying the number of repeated letters within a list. The greater the repetition, the greater the similarity. Each *S* learned three experimental lists, all at a constant level of similarity, and one practice list. In making the high similarity lists, only a few letters were used for each list; therefore, largely different groups of letters could be used for each list. Thus, while intralist similarity was high, interlist similarity was low. On the other hand, for low-similarity lists, repetition of letters within lists was low, which automatically produced high repetition among lists. The consequence of this is that experiments with low intralist similarity should produce high interference between lists, while for experiments with high intralist similarity the interference between lists should be relatively low. Furthermore, since the low-similarity lists have a greater number of different letters used in any one list than do high-similarity lists, we would expect that all previously extra-experimental material learned would interfere more with the low-similarity lists than with the high-similarity lists.

With the above considerations in mind, it can be seen that in the case of high-similarity lists there will be low interference from previously learned lists and previous experience, and high interference from within the lists. For lists of low similarity there will be high interference from previous lists and experiences, and low interference from within the lists. An explanation for the results that we have found (no appreciable differences in recall as a function of similarity) requires only that the total interference at recall be the same for all levels of similarity. The present explanation does indeed assume that this interference is equal, but that the sources of this interference vary as indicated above. Thus, intralist similarity has not appeared to be a recall variable because intralist similarity is reciprocally related to interlist similarity.

SOME SUPPORTING EVIDENCE

Overt errors at recall. In the present experiments we find that total errors at recall are roughly equal for the various levels of intralist similarity for a given material. More important, however, for the present hypothesis is the analysis of the sources of these errors. According to the hypothesis advanced above, the reciprocal relationship between interlist and intralist similarity has prevented recall from being inversely related to intralist similarity. With low intralist similarity we assume that such interference occurs at recall from other previously learned lists; with high intralist similarity we assume that interlist interference is low but intralist interference high. Therefore, if we examine each error as to its source, a fairly direct test of the hypothesis can be made. Such an examination can be made for experiments in which variation in intralist similarity has been accompanied by differences in rate of learning or at least by differences in overt error frequency. We have analyzed error sources for all of our experiments which meet these criteria.

The findings show that total number of errors at recall is about the same for all levels of intralist similarity (for a given material and method of learning), but that the source of errors shifts as a function of this similarity. We cannot, of course, present all of these data. In all cases, differences in source of errors were clear between low and high intralist similarity, but the medium-similarity conditions did not always fit neatly between the high and low. As an illustration of the clearest findings, we will present the combined results for five paired-associate syllable experiments (8).

In dealing with errors in syllable experiments, we find that the source of some errors cannot be determined, probably because the response represents a combination of two or more syllables. There are also partial errors (one or two letters), the source for which cannot be determined; we have omitted these from present tabulations. Finally, the total errors is actually rather small considering the number of *S*s involved (three recall periods for each of the 180 *S*s). In spite of these shortcomings in the data, the results, as seen in Table 1, show very clear trends.

TABLE 1

SOURCE OF OVERT ERRORS AT RECALL AS A FUNCTION OF INTRALIST SIMILARITY

Intralist Similarity	Source of Error						Total
	List Being Recalled		Other Lists		Unidentified		
	N	%	*N*	%	*N*	%	
Low	54	25	100	47	30	28	214
Medium	90	39	58	25	81	35	229
High	94	46	26	13	85	41	205

Note first in Table 1 that total errors at recall is roughly equal for all three levels of intralist similarity. The second fact to note is that, as intralist similarity increases, errors from within the list increase. That is, the error made was a correct response for some other stimulus within the list. The third fact of importance is that errors from other previously learned lists *decrease* as intralist similarity increases. The unidentified errors tend to increase with intralist similarity, but this has not been found consistently in other experiments. In short, Table 1 gives strong support to the idea that intralist and interlist similarity are reciprocally related and that this relationship is responsible for failure to find consistent trends in recall as a function of intralist similarity.

Recall as a function of stage of practice. On the basis of the hypothesis, we would predict that decrement in recall

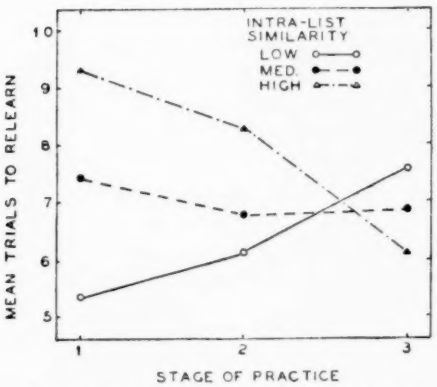

FIG. 1. Relearning of paired-syllable lists as a function of stage of practice and intralist similarity

as a function of stage of practice would be greater for low-similarity lists than for high-similarity lists. The reasoning is that, with each successive list learned, more interferences should be present for the low-similarity lists than for the high-similarity lists. By stage of practice is meant that, when *S* serves in more than one condition, each successive condition represents a greater or higher stage of practice. Our data do not allow a good test of this deduction because (*a*) the high-similarity lists obviously have some repetition of letters among them, and (*b*) all *S*s in all experiments had a practice list of syllables before starting the experimental sessions. Both of these factors would tend to diminish differences in recall as a function of stage of practice between low- and high-similarity lists. Nevertheless, our data for nonsense syllables support this deduction, but in no case is there satisfactory statistical reliability. An example will illustrate these trends. For paired nonsense syllables in which stimulus similarity was varied, the recall for three successive stages of practice for the high-similarity lists in terms of mean number of syllables correctly recalled was 5.06, 5.06, and 5.06 (a coincidental "proof" of the null hypothesis). For low-similarity lists the corresponding values were 4.56, 3.92, and 3.31. According to the hypothesis, this gradual decrement for the low-similarity lists is to be attributed to increasing interlist interference.

Relearning as a function of stage of practice. The most convincing evidence of the differential effect of previous learning on retention of lists of different degrees of intralist similarity is shown in the relearning data for the paired-associate lists. Figure 1 shows the mean number of trials to relearn lists as a joint function of stage of practice and intralist similarity. These data are from paired-associate syllables with variation in stimulus similarity. Statistically, the interaction between stage of practice and intralist similarity is highly significant. On the first stage of practice there are large differences in relearning as a function of intralist similarity, but these differences reduce rapidly so that by the third stage of practice the differences are actually reversed. This interaction, according to the hypothesis, occurs because the relearning of high-similarity lists is less interfered with by previous lists than is the re-

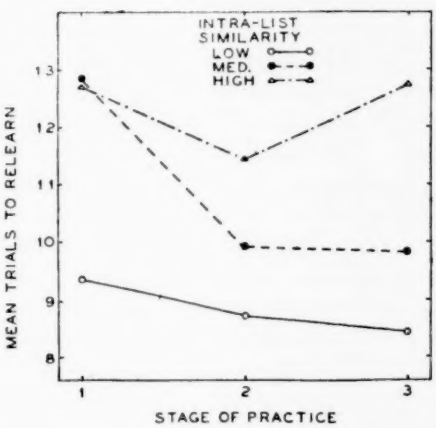

FIG. 2. Relearning of serial-syllable lists as a function of stage of practice and intralist similarity

learning of low-similarity lists. Comparable curves were found for the experiments with paired syllables in which intralist response similarity was varied. But, when we plotted corresponding curves for serial nonsense syllable learning, we found that there is no appreciable interaction between intralist similarity and stage of practice. These curves are shown as Fig. 2. While trials to relearn and intralist similarity are directly and significantly related, we see no evidence of the marked interaction between stage of practice and intralist similarity which was present in the paired-associate list.

Recall results for serial-syllable lists and paired-syllable lists showed quite the same results, namely, no difference as a function of intralist similarity. But, as seen in Fig. 1 and 2, marked differences in relearning occur for these two kinds of learning. These differences between paired-associate and serial learning raise two explanatory problems which must be handled if the general hypothesis under consideration is sound: First, why is interlist interference less in relearning serial lists than in relearning paired associates? Second, since the interference for serial lists is less than for paired-associate lists, why should not the serial lists of high intralist similarity show a greater loss at recall than lists of low similarity? This last question may need further clarification. The hypothesis holds that failure to find differences at recall as a function of intralist similarity is because the reciprocal relationship between interlist and intralist similarity varies. Now, however, it is being maintained that interlist interference is less in serial learning than in paired-associate learning because of the evident differences in Fig. 1 and 2. Therefore, in serial learning, intralist similarity should be the dominant factor in recall (according to the theory), but still we do not get poorer recall with high-similarity lists than we do with low-similarity lists. We shall attempt to handle these two questions in the order stated.

In paired-associate learning, items are presented in a different order on each trial. Each pair, therefore, is to a certain extent an isolated learning task, and the constant stimulus cues for one response may be quite different from those for another. In serial learning, on the other hand, there are cues which are constant for the list as a whole. The entire list is tied to a common initial stimulus, and the series of visual and response-produced cues present throughout the list may remain relatively constant from trial to trial. Because of the unique and constant cue complex present in a serial list, gross differentiation of one serial list from another would be better than the differentiation of one paired-associate list from another. By gross differentiation is meant that there are cues, especially the cues provided by the first syllable of the list, which allow *S* to identify the list as a list. Phenomenologically speaking, these cues allow *S* to say in effect, "Oh, yes, *this* list." Because of the lack of cue constancy for a list as a whole in paired-associate learning, such differentiation is less readily achieved. In short, it is the hypothesis that serial lists are more resistant to interlist interference than are paired-associate lists because of the greater number and constancy of differentiating cues. It may be likened empirically to experiments (e.g., Bilodeau and Schlosberg, 1) in which it is shown that retroactive inhibition is decreased if the original and interpolated lists are learned in quite distinctive environments.

Turning next to the second question asked above, we discover that a precise answer is difficult. The question arose because, if the results suggest that inter-

list interference is less in serial learning than in paired-associate learning, why then should not intralist similarity show an inverse relationship with recall of serial lists? The answer, it is believed, lies in the fact that interference produced by the combination of interlist similarity and serial position is very transitory in serial lists. It is known from analyses made by Melton and Irwin (5), and Melton and von Lackum (6), that interference which might be attributed to serial position is very transitory in the retroactive inhibition paradigm, and, furthermore, "that the serial position factor is not sufficient to produce a significant number of intrusions unless there is also a formal similarity of the materials learned" (6, p. 170). It is believed, therefore, that interlist interference in recall of serial lists in our experiments is produced by a combination of formal similarity and serial position, this interlist similarity being greater for lists of low intralist similarity than for those of high intralist similarity. The transitory nature of this interference, we believe, is due to the fact that the unique cue complex of a serial list quickly re-establishes differentiation between the list being relearned and previous lists. Once this is re-established, relearning rates are largely determined by intralist similarity. In our serial nonsense syllables experiments, time to relearn and intralist similarity were directly related.[2]

[2] The concept of differentiation might also be used to account for differences in retention due to meaningfulness. Meaningfulness, according to the theory, largely acts a differentiating cue, and we would, therefore, expect less interlist interaction among meaningful material than among nonsense material. Also, the greater interference among paired-associate lists as compared with serial may account for the fact that apparent practice effect (learning-how-to-learn) is greater for serial lists than for paired-associate lists.

METHODOLOGICAL IMPLICATIONS

Our analyses have made evident the fact that there is considerable interaction among conditions when the same *S* serves in a series of conditions. In our experiments, a study of this interaction has been valuable in our attempts to understand factors influencing retention. But it must be apparent that unless this interaction among conditions is the major source of interest, we cannot recommend the counterbalanced type of design that we have used. Even so, we have no assurance that intralist similarity, for example, would, if other designs were used, show any different relationship with retention than that which we found. Suppose we sought to determine the influence of intralist similarity on recall of serial nonsense lists by means of a situation in which each condition of similarity was allotted to a different group of *S*s and no practice day was used. Under such a situation, the influence of intralist similarity would be studied without any bias produced by other experimental learning, and the inverse relationship between intralist similarity and retention as predicted by theory might be shown clearly. Whether or not such a relationship would emerge depends, we believe, upon the interference produced by extra-experimental experiences. Gibson (3) used such a design with paired associates which had nonsense figures as stimuli and syllables as responses, and failed to find significant differences in recall. It is therefore questionable whether or not such a design will demonstrate the relationship expected. It appears that intratask similarity cannot be studied without confounding by its reciprocal relationship with intertask and extratask similarity. If we use highly meaningful material and vary intralist similarity, the interaction with other learning is considerably reduced;

but, for such material, facilitative cues for retention appear to increase directly with intratask similarity (10). In short, we do not believe that a change in the design of experiments will appreciably change the facts we have presented.

SUMMARY

Research has shown that intralist similarity of verbal materials is not related to retention as measured by recall. This fact is in apparent contradiction to theory and to the well-known fact that similarity between lists is systematically related to recall. A general hypothesis was developed to account for the findings in a manner that would be consistent with specific interference theory and fact. The hypothesis first assumes that when lists varying in intralist similarity are equally well recalled, the interference is equal. Second, it assumes that interference which can be attributed to intralist similarity alone increases directly with intralist similarity. Third, it assumes that increases in intralist similarity are accompanied by decreases in interlist similarity. Therefore, as interference due to intralist similarity increases, interference due to interlist similarity decreases. The total interference at recall could then be roughly equivalent for all levels of intralist similarity; but the source of the interference varies. Several deductions concerning sources of interference were tested, and considerable support was found for the general hypothesis.

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OPERATIONAL DELINEATION OF "WHAT IS LEARNED" VIA THE TRANSPOSITION EXPERIMENT

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In a recent issue of this journal, Howard H. Kendler (15) has offered the challenging suggestion that the question of "what is learned" is a pseudo problem, a theoretical blind alley. Because psychology abounds in perennially argued problems which seem to survive experimental attack, such a suggestion was certain to receive much attention. This indeed has proven to be the case with two already published rebuttals (25, 26) and much favorable attention, which naturally has not yet appeared in the literature. In continuing the discussion of this important topic, it is the purpose of the present paper to analyze Kendler's proposition into two parts, the first of which seems eminently acceptable and the second of which is found to be untenable.

Kendler's first point may be stated as follows: All perceptual, cognitive, and learning theories are response theories. For all of them, the intervening variables involved are to be inferred from the responses made by the organism in specified situations. While this point has been made before, it is one that is well worth making repeatedly and one which is often forgotten in the current resurgence of perceptual and phenomenological theorizing in the areas of social and personality psychology. Its neglect has led to a tendency to overlook the possibility of functional equivalence between perceptually stated intervening variables and those formulated in response terms, and thus in a number of instances has led to the generation of pseudo problems. To illustrate, the classic study (18) on the relative inability of anticommunists to learn procommunist materials, and vice versa, initially interpreted in terms of the perceptually filtering nature of an attitude, might equally well be interpreted as habit interference involving habits of favorable and unfavorable response to the stimulus, "communist." Not only is this alternate explanation tenable, but it furthermore seems unlikely that there would be any operations possible which would distinguish between the two in terms of their relative adequacy. In a similar fashion, the myriad of recent studies on the influence of need and value upon the perceptual thresholds for relevant stimuli (e.g., 3, 22, 24) could equally well be stated in terms of the response readinesses or response latencies involved. It seems quite probable here also that an operational distinction between an interpretation in terms of perceptual thresholds, and an interpretation involving response thresholds, will be impossible.

Or to take another illustration: It has been proposed (1, 9) that the traditional doctrine with regard to suggestion must be rejected, for it is not that the response has been changed by suggestion but rather that the view of the object has been changed. While in this instance introspective data are accumulated to buttress the distinction, it seems likely that for most situations, and certainly for all in which animals are used, the two interpretations would have no operational distinction.

[1] The preparation of this paper was made possible through the author's participation in the Behavioral Science Study Group, University of Chicago, 1952–1953.

As a final illustration, let us quote from Bruner's (2) recent statement of an "outline of a theory of perception." Note how closely his principles are approaching those of habit formation, and what little incongruity would occur if the term *habit* were substituted for the term *hypothesis:*

> The stronger a hypothesis, the greater its likelihood of arousal in a given situation. . . . The greater the strength of a hypothesis, the less the amount of appropriate information necessary to confirm it. . . . The greater the strength of a hypothesis, the more the amount of inappropriate or contradictory information necessary to infirm it. . . . The more frequently a hypothesis or expectancy has been confirmed in the past, the greater will be its strength. . . . The smaller the number of alternative hypotheses held by the person concerning his environment at a given moment, the greater their strength will be. . . . The larger the number of supporting hypotheses . . . the stronger the hypothesis. . . . The more basic the confirmation of a hypothesis is to the carrying out of a goal striving activity, the greater will be its strength (2, pp. 126–127).

It is easy to see in these principles the habit acquisition principles of frequency, effect, and resistance to extinction, as well as postulates regarding habit competition. It is also very difficult to imagine any operational way of distinguishing between the utilization of a concept of habit versus one of hypothesis in such formulations. It is also apparent that the development of such parallels without explicit recognition of potential equivalence is contributing needlessly to the surfeit of concepts and schools with which psychology is cluttered today.

While thus accepting and reinforcing Kendler's first point, it is only fair to point out that Tolman, for one, has been aware of the fact that all intervening variables in cognition and learning theories are inferred from responses. And while Kurt Lewin seemed inexcusably vague on the source of the data from which the life space was inferred, it even seems possible that he was from time to time aware of this point: "In other words, a given state of a person corresponds to a variety of behavior and can therefore be inferred only from a combined determination of overt behavior and the situation," or "the observation of behavior permits not only the determination of conscious goals but also of unconscious goals as Freud uses the term" (19, pp. 793, 806).

The second point in Kendler's analysis, as we see it, is that because all learning and cognition theories involve response-inferred intervening variables, the question as to "what is learned" is a pseudo problem. Since we agree with so much of what he says, it will require some pains to make explicit the point of disagreement. We would agree with him that the failure to recognize that perceptually or cognitively stated intervening variables are in fact response inferred will increase the likelihood of false distinctions and pseudo problems. We would also agree with him that just because theories differ in using or not using intervening variables—or in using phenomenologically stated intervening variables as opposed to physiologically conceived ones—there is no reason to anticipate any effective or operational difference between the theories. We would further agree with him that the extreme contrast in language utilized in characterizing intervening variables, when reinforced by pervasive tendencies toward reification in theoretical thinking, does lead theoreticians and their readers to assume superficially that theories differ when, in fact, they do not. But there still remains the possibility that two theories of learning, both utilizing response-inferred intervening variables, would differ in an experimentally meaningful fashion in terms of their

specifications of "what is learned." It will further be our point that, in part at least, the controversy over "what is learned," as it is epitomized by Guthrie and Tolman, has such a genuine core of empirical meaning, and that experiments are available relevant to this controversy.

The basic difficulty centers around a generally unrecognized ambiguity in the term "response." For most of the older discussions of learning, especially in the Thorndike and Pavlov traditions, it seems to be uncritically assumed that a response is an obvious "given," a term so primitive and fundamental that it does not require operational definition. In so doing, there is a tendency to choose one of several possible definitions as the "obviously" correct one, not recognizing that alternative interpretations exist or that there are operational ways of determining which is the most appropriate. Let us take, for a hypothetical example, a dog that, under reinforcement of electrical shock, has learned to lift its paw at the sound of a buzzer. The response can be defined in at least the following ways: (*a*) The dog has learned to contract certain muscles at the sound of a buzzer. (*b*) The dog has learned to change the position of its foot relative to the position of its other feet and its total body at the sound of a buzzer. (*c*) The dog has learned to remove its body from contact with a specific object. (*d*) The dog has learned to remove its body from a given location, defined in terms of the frame of reference of the room or in terms of latitude and longitude.

These, others, and certain combinations are all possible as interpretations of what is learned. They are also operationally distinguishable inasmuch as they place this response in a category of responses significantly different and they imply, for changed conditions, a different pattern of manifestation. But most important, these four and the possible others are experimentally confounded in this learning experiment as described, and in all learning experiments in which the habit is tested only in the situation in which it was originally acquired. Rather than assume one interpretation and neglect the possibility of others, it behooves the learning theorist who wishes to have his theory based upon firm empirical grounding to set up experimental situations in which these possible interpretations are disentangled and unconfounded. To parallel similar studies of the stimulus, such experiments may be called "transposition experiments." While they are all too few in the literature that serves as the primary buttress to the major learning theories, some are available and will be illustrated below.[2]

Four experiments on a conditioned finger movement done by Wickens (**31**, **32**, **33**, **34**) provide an excellent illustration of what is meant by a transposition experiment. Through pairing of a shock and a tone in a situation in which an extensor movement of the finger removed the shock, a conditioned finger movement was achieved. Among other alternatives, this response could have been interpreted as an extensor movement of the finger or as a withdrawal of the finger from the electrode. As long as the learning or conditioning is tested only in this initial situation, these two interpretations remain confounded. However, in the four experiments referred to, Wickens followed up this initial training by turning the hand over. In the new situation, the two interpretations previously confounded

[2] In what follows, the present writer is making some of the same points raised by Smedslund (**26**) in his extremely condensed critique of Kendler's paper. Like Smedslund, the present writer is indebted to Egon Brunswik (e.g., **4**, **5**) for much of the orientation presented.

could now be experimentally disentangled in that withdrawal of the finger would now involve a flexor rather than extensor finger movement. The four experiments varied among themselves as to the tones presented and the preconditioning variables, but are sufficiently comparable so that their results may be combined for the present purpose. Of the 131 subjects involved in all, 32 showed no response generalization, 8 showed responses of the extensor sort, indicating that the variance which appropriately characterized the acquired habit was an invariance of muscular contraction. Ninety-one of the cases showed a flexor movement, indicating that the invariance which characterized the habitual response was to be defined in terms of its effect in changing the organism-environment relationship. For the subjects as a whole, the term "withdrawal" was more appropriate as a characterization of the response than the term "extensor movement."

While 90 per cent of those whose habit was testable through generalization to the new situation showed the withdrawal type of response, it is worthy of note that both types occurred and that this complicates the theoretical implications, even though one group is in a small minority. It is equally important to note that the type of response thus operationally delineated was not the definition of the response implicitly assumed by the research tradition in which Wickens was participating. Thus the title of his first paper, "The Transference of Conditioned Excitation and Conditioned Inhibition from one Muscle Group to the Antagonistic Muscle Group" (31), contains within it the assumption that what had been learned was a conditioned excitation and conditioned inhibition specific to a muscle group; that the response was appropriately defined in terms of muscle contraction. In terms of the present perspective and in congruence with at least part of Wickens' own interpretations, it could more accurately be said that, rather than demonstrating the transference of a muscle-defined habit, Wickens instead had demonstrated that what had been learned was not a muscle-defined response. Rather than studying transfer of a response the essential characteristics of which were already known, Wickens' research must be regarded as operationally delineating the appropriate dimension of response characterization. Until the transposition experiment had been conducted, Wickens did not know "what the response was" in such terms as to be able to give it a label appropriate to the invariance that would be found in various expressions of the habit. The label "conditioned excitation of a muscle group" was at the very least presumptive, and, as it turned out, incorrect.

As a second illustration of a transposition experiment, the excellent paper by Nissen, "Description of the Learned Response in Discrimination Behavior" (23), may be cited. In reply to the mathematical theories of discrimination learning provided by Gullicksen and Wolfle (10) and by Householder (14), in which the response had been defined as jumps to the right or to the left rather than movements toward or away from the discrimination stimuli, Nissen transposed the stimuli from a left-right alternating position to an up-down alternation, and demonstrated a high degree of transfer of the discrimination habit to this situation (as would indeed be predicted by common sense, but not by the conscientious mathematical theories in question, which had specified the response in terms of muscle or body parameters). To quote from Nissen's conclusion: "The logical necessity, in some instances, of describing the discriminative response as an act, that is, in terms of its consequence or effect

(usually with reference to a goal or an altered organism-environment relationship), is illustrated in the ape experiment reported above. Description in terms of movement, without reference to the environment, does not account for the prompt transfer shown by sixteen chimpanzees" (23, p. 130).

We may interpret as transposition experiments in the present context a group of experiments conducted in the service of checking Watson's theory that maze-running habits consisted of chained reflexes keyed off by, or connected by, kinesthetic sensations. All involved observing responses in altered maze conditions. With the exception of the initial one in the series (6), the others (8, 16, 17, 21, 29) seem to have effectively disproven Watson's hypothesis, making it one of the few theories in psychology to die at the hands of crucial experiments. While the issue in question was tangential to the present one, some of the experiments are relevant in the present context. These have in common the attribute that they modify the conditions under which the habit is executed in such a fashion that a muscle- or body-consistent definition of the learned response in the habit cannot be held if the animal continues to perform adequately. More precisely, they modify the conditions for the execution of the habit in such a fashion that, if the acquired habit is in fact an invariant of motor responses defined in terms of muscle or body parameters, no learning or transfer will be manifest in the new situation. For our present purposes they are not as symmetrically appropriate as Wickens' experiments inasmuch as they provide no clear opportunity in the novel situation for the observation of possible invariance of learned response in terms of muscle or limb movement parameters.

We may consider first two experiments by Lashley (16, 17). In these studies, spinal or cerebellar operations performed upon animals that had learned a maze so disturbed their motor coordination that their manner of running through the maze was greatly different from what it had been during the initial learning. In some instances the rat had to proceed in a rotating or somersaulting fashion; in other instances it had to drag along paralyzed hind limbs. Under these conditions the animals were still capable of running the maze in an apparently errorless fashion, an achievement which may be interpreted for the present argument as indicating that the responses that had been learned in achieving the maze habit were not to be defined in terms of specific limb or muscle movements, but rather were to be determined in terms of achieved locomotion. Dennis (8) provided a transposition experiment by removing the walls of a maze after the rats had learned a habit. Seeing rats ran directly to the food box, manifesting a learned response that was invariant only when conceived of as an achieved locomotion or change of organism-environment relationships. Conceiving of the habit as an invariance in motor movement would not appropriately predict the behavior of the rat in this changed situation, nor would it be relevant to the learning which the animals obviously continued to show. An experiment by Macfarlane (21) provides the final illustration from this series. Macfarlane trained groups of rats to swim through a spatial maze and then tested their retention of the problem when they were required to wade, and vice versa. A successful retention of the maze habit was found, indicating that the essential responses learned cannot be defined in terms of motor movements, but must include reference to achieved locomotion in environmentally defined terms.

Among some of those who have worked within the traditional condi-

tioning framework, doubt as to the adequacy of defining the response in muscle parameters has arisen. Thus Liddell (20), Hilgard (13), and Culler (7) have all noted that the conditioned response tended to differ from the unconditioned response in an adaptive direction, becoming in many instances more nearly an "avoidance reaction" than a repetition of the unconditioned response. While the conditions of these experiments have not provided transposition experiments of a clear-cut nature, their evidence is relevant to the operational delineation of "what is learned" in terms of the two principal competing operational definitions. At least one study in the conditioning literature goes further: in 1932 Warner published an article, "An Experimental Search for the Conditioned Response" (30), which represented an effort to explore the syndrome of response consistency which had been created through the conditioned response, and ended up with the notion that this consistency had to be described as a generalized tendency to escape by any means rather than as a definite movement.

So far in our discussion of actual or approximate transposition experiments, the results have piled up overwhelmingly on the side of interpreting the learned response in habit formation in terms of organism-environment shifts rather than in terms of limb or body movements defined in terms of body parameters. The minor exceptions to this are the 9 per cent of Wickens' subjects whose responses were consistent with the latter definition, and a few of the tracings of blinded rats in Dennis' study. The careful observations of Guthrie and Horton (12) on cats in a puzzle box provide the major case for the operational definition of the learned response in movement terms. They present a plausible case for the occurrence of a great deal of specific movement response in learning, and legitimately point out that, in the normal method of recording maze or puzzle box behavior, evidence as to this movement specificity is neglected in favor of recording in terms of achievement units. Most of their data are in the form of repetition of similar approaches to escape on the part of single animals and variability between animals in a puzzle box where a wide variety of movements could effect the release. However, they do report incidentally, if not systematically, on some genuine transposition experiments. In their puzzle box, release was achieved by tilting a flexible upright post. For the animals showing the stereotyped movement responses, this tilting often was achieved incidentally by tail or hind leg contacts while the animal was making direct attacks on the door. When in some of these cases the location of the pole was shifted, the animal ineffectively repeated its stereotyped movement at the location where the pole had been. Similar movement consistency was occasionally observed on the part of animals who happened to enter the puzzle box on a changed heading. Precise tabulations on these points were not made available, and the experimental shifting of the pole apparently was not carried out in a systematic fashion. It is also mentioned that, for animals who had hit upon biting or attacking the pole as a mode of release, the movement of the pole did not interfere with the continuance of the adaptive response.

As Guthrie recognizes, the great bulk of the cats' behavior in the puzzle box is most easily described in terms of acts and not movements. The natural escape efforts addressed toward the screened top and the door have clearly an object consistency rather than a movement consistency insofar as they represent habitual or consistent responses. Indeed, it is because of the

dominating focus of the animal on the door and the cover that its escape is so frequently effected by inadvertent movements incidental to directed or object-consistent responses. It is possible that the movement consistency found by Guthrie is a function of this aspect of the puzzle situation. However, Guthrie would explain the behavior in the reverse fashion, claiming that the animal's spontaneous escape efforts are instances of exercising well-learned acts, which, on final analysis, are consistencies at the movement level embedded in habits learned in previous situations.

The studies of Tolman, Ritchie, and Kalish (e.g., 27, 28) on response versus place learning form the most recent examples. For a number of reasons, however, they contribute ambiguously to the present discussion. While their rate-of-learning findings argue against a body-parameter definition of the learned response, the few transposition experiments incidentally introduced (27, 28) indicate clearly that some rats, at least, were naturally tending to learn body-consistent responses.

It is noteworthy that, among learning theorists, Guthrie and Tolman—both of whom Kendler cites as emphasizing the importance of the "what is learned" problem—have likewise been clearest on the operational specifications of the learned response as their theories dealt with it. It also seems demonstrated that, even when one agrees that all intervening variables in learning or cognition theory are response-inferred, there still remains room for theories to disagree in an empirically meaningful fashion, and that, indeed, the problem has been put to empirical test. At the empirical level it is obvious that all the required evidence is not yet in, and that the empirical data (as well as the theoretical models) are at the moment in apparent conflict. The present writer is strongly of the opinion that Tolman and Guthrie are correct in their assumption that different learning theories imply different operational definitions of the learned response. If this is so, it behooves the learning theorist to go back to those principal experiments upon which his theory is based and, by operational manipulation of the situation of habit testing, discover for himself what kind of response definition fits the habits acquired, rather than to assume on demonstrably unsound grounds that the nature of the learned response is obvious and in no need of operational delineation.

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THE CASE FOR PERCEPTUAL DEFENSE

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The study of perceptual behavior from the viewpoint of personality theory has engendered considerable interest and controversy. While the influence of certain motivational and personality variables upon perception has seemingly been well established by several careful experiments (15, 16, 20, 31, 32), the concept of "perceptual defense" is still a confused and controversial issue. Several recent articles and experiments (3, 17, 18, 24, 33) have been quite critical of the perceptual defense concept. In attacking the concept,, however, most critics have failed to analyze the concept in detail, and, in general, have considered only a biased summary of the evidence in its favor. Consequently, the criticism has further confused an already muddled issue.

The present paper has three objectives. The first is to examine briefly the rationale for the general concept of psychological defenses. This examination is necessary in order to determine the methodological requirements for an adequate test of the perceptual defense hypothesis. The second objective is to consider the experimental data on perceptual defense in terms of these methodological requirements. And the third is to show that the phenomena of perceptual defense can be handled by experimentally acquired knowledge, free of the taint of mystical and mentalistic implications.

The term "perceptual defense" was introduced by Postman, Bruner, and McGinnies (29) as a perceptual principle to account for variations in the recognition thresholds for tachistoscopically presented value words. They found that a subject's recognition thresholds for words representing different value areas on the Allport-Vernon Study of Values test were inversely related to his score in the different value areas of the test. For example, a subject with high religious and low economic values as measured by the Allport-Vernon test tended to have a low threshold for religious words and a high threshold for economic words. The concept, perceptual defense, was invoked to account for the high thresholds for words representing a subject's low value areas while perceptual sensitization was posited as a principle to account for the lowering of thresholds for words from high value areas.

In labeling their principle perceptual defense, Postman *et al.* were drawing upon the general concept of psychological defenses as used in the areas of personality and psychopathology. Thus the implication of their report is that psychological defenses can and do influence perceptual recognition thresholds for certain kinds of stimuli.

In order to evaluate this possibility, it is necessary to consider briefly the role that the concept of defense has in theories of personality and psychopathology. Of the various theories in these areas, nearly all make use of the idea of defenses to some extent. Without going into a detailed theory of personality dynamics, one may say that defense mechanisms are conceived as learned ways of avoiding or dealing with anxiety that arises from certain specified sources. The sources specified in theory are "threats to the individual's self-esteem."

A basic assumption underlying the theory of defense concerns the ability of

the human organism to detect the presence of anxiety-arousing stimuli at an unconscious (nonverbalizable) level of awareness. This unconscious detection is necessary if defensive responses are to serve their hypothesized role of preventing the spread of anxiety to more conscious levels of awareness.

Fortunately such an assumption would appear to be on safe ground. Not only is the existence of different levels of awareness supported by common-sense observation, but it also receives support from diverse experimental data. Much of this evidence has been summarized by Miller (26).

More pertinent for our present problem of perceptual defense is the experiment by Lazarus and McCleary (22), subsequently confirmed (23, 35), which demonstrated that the subjects were able to discriminate among stimuli in terms of an autonomic nervous system response when the stimuli were exposed at speeds too rapid for verbal or conscious identification. Nonsense syllables to which a GSR had previously been conditioned produced significantly greater GSR's than nonconditioned nonsense syllables even though all the syllables were exposed at tachistoscopic speeds too rapid for verbal discrimination. Evidence of this type is crucial to the perceptual defense concept since it suggests a process whereby the organism can "know" what to defend against.

EXPERIMENTAL METHODOLOGY IN PERCEPTUAL DEFENSE

The general question of perceptual defense revolves around the question of whether certain kinds of perceptual behavior are related to behavior in other areas where it is generally characterized as "defensive." If it can be shown that perceptual recognition behavior is able to reflect defense mechanisms, then it becomes possible to use perceptual recognition as a means of bringing the general problem of defense mechanism under experimental scrutiny.

In order to test the perceptual defense hypothesis adequately, there are methodological requirements that the experiment must satisfy. It is necessary to show (*a*) that the stimuli to be perceived are anxiety provoking for the individual subject and (*b*) that the subjects have learned to deal with anxiety from this source by using avoidance defenses. As to meeting these requirements many of the experiments interpreted as demonstrating perceptual defense (7, 25, 29) and many of those refuting it (3, 33, 36) have been inadequate.

The experiment by Postman, Bruner, and McGinnies (29) which introduced the concept of perceptual defense was deficient in terms of these criteria. There was no evidence or reason to believe that words representing low value areas for a subject were anxiety-arousing. In fact, the data from this experiment could have been more economically explained in terms of their other perceptual principle, selective sensitization. That is, a low value area produced less sensitization than a highly valued area.

A number of experiments (3, 7, 25, 33) have used what may be called the "dirty word" procedure. This consists of exposing socially taboo words such as rape, bitch, belly, etc., together with socially more acceptable words, by a tachistoscope or tachistoscopic-like procedure. The subject's duration thresholds are then determined for the various words. If higher thresholds are obtained for the taboo words, this is interpreted as evidence for perceptual defense (7, 25), while lack of such a finding is considered as evidence against the hypothesis (3, 33). Such a procedure has a number of methodological pitfalls. As Howes and Solomon (17) have pointed out, such studies fail to

control for differences in the familiarity of the stimulus words. Also their interpretation is confused because of the possibility that the subject may deliberately delay his report owing to the embarrassment he would suffer from incorrectly reporting "whore" when the stimulus was actually "whom."

There is an even more fundamental criticism of the "dirty word" procedure. The methodology and logic of the procedure are such that the method as a whole is incapable of testing the perceptual defense hypothesis. The implicit assumption that the taboo words chosen are anxiety-arousing for all or even a majority of the subjects is extremely gratuitous. Even if this assumption were substantially correct, the studies using this procedure make no provision for individual differences among subjects in terms of how they respond to or handle this anxiety. If one wishes to determine whether psychological defenses can affect recognition thresholds, it would seem obvious that a first requisite is to show that the particular stimuli give rise to defensive behavior as determined by other independent criteria.

A number of experiments in the need-in-perception area have used a method that would seem to insure that the perceptual stimuli are emotionally meaningful to the individual subjects. The method has been to use an independent instrument or technique that is generally accepted as detecting areas of conflict and anxiety in the subject. Stimuli associated with these areas are then presented for tachistoscopic recognition along with stimuli found to be affectively neutral. One technique has been to use a word association test to determine psychological sore spots (4, 9, 10).

Bruner and Postman (4) found that words producing association disturbances had higher recognition thresholds than words with medium or short association times. Eriksen (9) found that the amount of association disturbance to aggressive, succorant, and homosexual words was positively related to the subject's recognition thresholds for scenes portraying aggressive, succorant, or homosexual activity.

Lazarus, Eriksen, and Fonda (21) administered a sentence completion test to a group of neurotic subjects in which some of the sentences were so structured as to permit aggressive or sexual endings. The subjects then listened to a series of sexual, aggressive, and neutral sentences presented against a noise background. Appreciable correlations were found between the two tests. For example, a subject who freely expressed sexual endings to the sentences in the sentence completion test perceived the sexual sentences in the auditory test at least as well as he perceived the neutral sentences. On the other hand, a subject who blocked or gave distorted endings to the sexual sentences tended to perceive considerably less of the sexual sentences than of the neutral sentences in the auditory test.

Similar findings were obtained by Eriksen (8) using the TAT to detect areas in the personality where defenses might be expected to occur. Here it was found that subjects who blocked or showed other indications of emotional disturbance to TAT cards which generally produced stories with aggressive themes had higher recognition thresholds for aggressive than for neutral scenes presented tachistoscopically. Those subjects, on the other hand, whose TAT stories freely expressed aggressive themes had relatively low recognition thresholds for aggressive scenes tachistoscopically presented.

The above studies demonstrate that impressive evidence can be obtained for a concept like perceptual defense when proper experimental precautions are taken to insure that the perceptual

stimuli to be used are indeed anxiety provoking for the individual subjects. Even more convincing evidence is obtained when the experimental procedure also takes account of individual differences in response to self-esteem threat. Several experiments have shown that there are marked and consistent differences between individuals in the way that they respond to threats to their self-esteem (1, 5, 10, 11, 12, 19, 30). Some subjects respond to the anxiety or threat by psychological avoidance. They tend to forget or perhaps "repress" stimuli associated with it. Other subjects show a sensitization to the anxiety-related stimuli. They appear to dwell on or ruminate about it. It seems reasonable to expect that perceptual defense is more apt to be shown by subjects who use an avoidance response to self-esteem threat than by those subjects showing the ruminating reaction. The validity of this expectation is confirmed by experimental work.

In an ingenious experiment Postman and Solomon (30) studied their subjects' recognition thresholds for words associated with success and failure experiences. The subjects were given ten anagrams to solve under circumstances that would be expected to involve their self-esteem (an intellectually competitive situation). The experimental arrangement was such that each subject could solve only half of the problems. After the time limit was up for each anagram the correct solution was announced and the subject was required to write the solution on his answer sheet. In a second experimental session the recognition thresholds were obtained for the anagram solutions. For the group of subjects as a whole, no significant difference was found in recognition thresholds between words representing successful solutions and those representing failures. However, for any given subject, recognition thresholds tended to be significantly higher for *either* success words *or* for failure words. Eleven of the 17 subjects had a mean difference in recognition thresholds between their success and failure words that was significant at or beyond the .05 level of confidence, while for three of the remaining subjects the differences were significant at about the .10 level. These results are quite clear in demonstrating individual differences in reaction to threats to self-esteem and also in showing the effects of these reactions upon perceptual recognition behavior.

Similar and confirmatory findings were obtained in an experiment by Eriksen (10). Here two extreme groups of subjects were selected, based upon whether they recalled more completed or more incompleted tasks when the tasks had been administered under the guise of an intelligence test. Both groups were given a word association test and at a later date recognition thresholds were obtained on each subject for 25 words on which he had had widely different association times. All subjects in the completed task recall group had significant positive correlations between their association times and recognition thresholds for words, while the correlations for the subjects in the incompleted task or failure recall group were of zero order. In other words, perceptual defense was obtained only for subjects who had previously shown an avoidance type of defense in the memory test.

Both of the above experiments are quite clear in showing that an adequate test of the concept of perceptual defense must be prepared to take account of individual differences in defensive reactions. Lazarus, Eriksen, and Fonda (21) have supported this point in reporting that differences in psychological defenses observed at the clinical level of description lead to different perceptual effects. Patients character-

ized as using an avoidance, repressive type of defense were found to have higher recognition thresholds for anxiety-arousing stimuli than patients described as using intellectual or rationalizing defenses.

CRITICISMS OF THE PERCEPTUAL DEFENSE HYPOTHESIS

The reviews and experiments that have criticized the perceptual defense concept have not taken into consideration the studies summarized above. Howes and Solomon (17), Luchins (24), Postman, Bronson, and Gropper (33), and Bitterman and Kniffen (3) have been primarily concerned with the evidence from the "dirty word" type of experiments, which, as we have seen above, are fundamentally incapable of testing the perceptual defense hypothesis.

There are, however, several general criticisms that have been advanced against the perceptual defense hypothesis that need to be considered. Solomon and his associates (17, 36, 37) have presented experimental evidence indicating that the familiarity of a stimulus, defined in terms of its frequency of prior occurrence, is a factor in determining the recognition thresholds for the stimulus. It has been argued elsewhere (13) that a re-examination of the data suggests that frequency alone plays a relatively minor role in perceptual recognition. What effect it does have seems to be in the limited range of very low frequencies of prior occurrence. In the case of English words, variation in frequency beyond a frequency range of ten in a million in the Thorndike-Lorge count (38) appears to bear little if any relationship to ease of tachistoscopic recognition. Similar findings are obtained with words from artificial vocabularies.

Even if familiarity could be shown to be a major determinant of recognition thresholds, it still would be unable to explain the evidence for perceptual defense. It is difficult to see how one could use familiarity to explain why some subjects have high thresholds for words when these words are associated with a failure experience, and other subjects have low thresholds for the same words when they are associated with success experiences, as reported by Postman and Solomon (30). By the same token an explanation in terms of familiarity would be hard put to explain why subjects who recall completed tasks have high recognition thresholds for words with long association times, while subjects who recall incompleted tasks do not (10).

A second general criticism concerns the question of whether subjects deliberately and consciously withhold their verbal report for certain stimuli. This is, of course, a vexing problem that plagues a number of fields of psychological research. As applied by Howes and Solomon (17) to McGinnies' experiment (25), it is a fair criticism. In his experiment McGinnies used the "dirty word" procedure, and there is a reasonable question as to whether the higher thresholds obtained for the taboo words were not because of the subject's greater conscious hesitation to guess a taboo word to a stimulus than to guess a neutral one.

This criticism is less justified when applied to the experiments cited above in support of the perceptual defense hypothesis. In most of these experiments precautions were taken to minimize any hesitancy on the subject's part to report or guess. In addition it is difficult to see how this criticism can be applied to the previously discussed experiments of Postman and Solomon (30) and Eriksen (10), since in these experiments the perceptual stimuli were free of social taboos.

There have been several very general criticisms of need-in-perception experiments as a whole and of perceptual defense in particular. The main tenor of Luchins' criticism (24) has been the failure of research in this area to follow the "party line" of Gestalt psychology. He has also suggested that the concept of perceptual defense could be handled in terms of "set," and this is quite probably true. The concept of set has shown itself in the history of psychology to be both broad and ambiguous enough to hide many important problems.

Another objection has been against calling research in this area "perception." It is, of course, hard to draw any hard and fast lines between perception and general cognitive functioning. Little is to be gained by definitional squabbles. The important thing is to demonstrate the existence of a particular phenomenon and then to analyze it experimentally. In this manner operational definitions will eventually eliminate misunderstandings.

A Possible Explanation for Perceptual Defense

When the broad experimental basis for the perceptual defense hypothesis is properly considered, there seems little doubt that psychological defense mechanisms as conceived in personality and clinical terms can influence perceptual recognition behavior. There is still, however, another question to be considered. Can the behavior clinically described as defensive, as well as the behavior labeled perceptual defense, be reduced to an explanation in accordance with more familiar and generally accepted psychological processes? The available data are probably still too meager for any comprehensive theory of defense, but there are some suggestive explanations for perceptual defense that do not have to depend upon mystical soul-like concepts, as some have feared (18).

One possible explanation is in terms of the reported effects of anxiety upon problem solving and other cognitive functioning. Tolman (39) has discussed the possibility that anxiety acts to narrow the individual's cognitive map, and experimental data have indicated that anxiety retards learning (27) and increases rigidity in problem solving (2, 6, 34). Postman and Bruner (28) have further shown that stressing subjects by failure reports and derogatory comments results in higher perceptual recognition thresholds and a general deterioration in perceptual performance.

In many respects the perceptual recognition task can be viewed as essentially a problem-solving situation. In a tachistoscopic exposure the subject receives a few fragmentary cues from which to reconstruct the entire stimulus. If anxiety may be assumed to interfere with the availability and flexibility of hypotheses, then it is to be expected that stimuli provoking anxiety may require more cues before correct recognition occurs. Such an explanation depends quite heavily upon the assumption that the human organism can discriminate among stimuli at a nonconscious level of awareness, but as we have seen above, there is considerable experimental support for such an assumption.

To account for perceptual defense in terms of the effects of anxiety on cognitive functioning is only a partial explanation. There are still the interesting and consistent individual differences in the effects that anxiety has on perceptual recognition. As we have seen above, some individuals show perceptual defense for anxiety-arousing stimuli, while others do not. Whether this is indicative of differences in susceptibility to anxiety or to differences

in learned ways of dealing with it is a problem for further research. Already there are some indications (**5**, **12**, **21**) that these individual differences are associated with rather broad dimensions of personality such as hysteria-psychasthenia and/or the extraversion-introversion factor reported by Eysenck (**14**). Conclusions, however, are still premature. The explanation presented here is not intended as a theory of perceptual defense. Its sole purpose is to show that the perceptual defense hypothesis is commensurate with and supported by experimental data obtained from diverse fields and sources.

SUMMARY

The present paper has been concerned with an evaluation of the concept of perceptual defense. Two experimental requirements were shown to be basic to any experimental test of the perceptual defense hypothesis. By use of independent instruments or techniques, it is first necessary to show that the perceptual stimuli are anxiety arousing for all subjects, and second, that the subjects have avoidance defenses available for anxiety arising from these sources.

Many of the experiments, both pro and con, on the question of perceptual defense were found to be seriously inadequate in terms of these criteria. On the other hand, experiments which attempted to meet these criteria have presented convincing evidence for the perceptual defense hypothesis.

A possible explanation for the perceptual defense phenomena was offered in terms of the previously demonstrated effects of anxiety upon problem solving and other cognitive functioning.

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SOME INFORMATIONAL ASPECTS OF VISUAL PERCEPTION

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The ideas of information theory are at present stimulating many different areas of psychological inquiry. In providing techniques for quantifying situations which have hitherto been difficult or impossible to quantify, they suggest new and more precise ways of conceptualizing these situations (see Miller [12] for a general discussion and bibliography). Events ordered in time are particularly amenable to informational analysis; thus language sequences are being extensively studied, and other sequences, such as those of music, plainly invite research.

In this paper I shall indicate some of the ways in which the concepts and techniques of information theory may clarify our understanding of visual perception. When we begin to consider perception as an information-handling process, it quickly becomes clear that much of the information received by any higher organism is *redundant*. Sensory events are highly interdependent in both space and time: if we know at a given moment the states of a limited number of receptors (i.e., whether they are firing or not firing), we can make better-than-chance inferences with respect to the prior and subsequent states of these receptors, and also with respect to the present, prior, and subsequent states of other receptors. The preceding statement, taken in its broadest implications, is precisely equivalent to an assertion that the world as we know it is lawful. In the present discussion, however, we shall restrict our attention to special types of lawfulness which may exist in space at a fixed time, and which seem particularly relevant to processes of visual perception.

[1] The experimental work for this study was performed as part of the United States Air Force Human Resources Research and Development Program. The opinions and conclusions contained in this report are those of the author. They are not to be construed as reflecting the views or indorsement of the Department of the Air Force.

The Nature of Redundancy in Visual Stimulation: A Demonstration

Consider the very simple situation presented in Fig. 1. With a modicum of effort, the reader may be able to see this as an ink bottle on the corner of a desk. Let us suppose that the background is a uniformly white wall, that the desk is a uniform brown, and that the bottle is completely black. The visual stimulation from these objects is highly redundant in the sense that portions of the field are highly predictable from other portions. In order to demonstrate this fact and its perceptual significance, we may employ a variant of the "guessing game" technique with which Shannon (17) has studied the

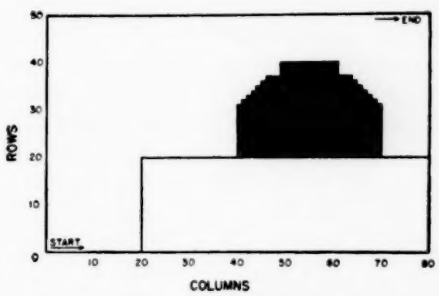

FIG. 1. Illustration of redundant visual stimulation

redundancy of printed English. We may divide the picture into arbitrarily small elements which we "transmit" to a subject (S) in a cumulative sequence, having him guess at the color of each successive element until he is correct. This method of analysis resembles the scanning process used in television and facsimile systems, and accomplishes the like purpose of transforming two spatial dimensions into a single sequence in time. We are in no way supposing or assuming, however, that perception normally involves any such scanning process. If the picture is divided into 50 rows and 80 columns, as indicated, our S will guess at each of 4,000 cells as many times as necessary to determine which of the three colors it has. If his error score is significantly less than chance [$2/3 \times 4{,}000 + 1/2(2/3 \times 4{,}000) = 4{,}000$], it is evident that the picture is to some degree redundant. Actually, he may be expected to guess his way through Fig. 1 with only 15 or 20 errors. It is fairly apparent that the technique described, in its present form, is limited in applicability to simple and somewhat contrived situations. With suitable modification it may have general usefulness as a research tool, but it is introduced into the present paper for demonstrational purposes only.

Let us follow a hypothetical subject through this procedure in some detail, noting carefully the places where he is most likely to make errors, since these are the places in which information is concentrated. To begin, we give him an 80 × 50 sheet of graph paper, telling him that he is to guess whether each cell is white, black, or brown, starting in the lower left corner and proceeding across the first row, then across the second, and so on to the last cell in the upper right corner. Whenever he makes an error, he is allowed to guess a second and, if necessary, a third time until he is correct. He keeps a record of the cells he has been over by filling in black and brown ones with pencil marks of appropriate color, leaving white ones blank.

After a few errors at the beginning of the first row, he will discover that the next cell is "always" white, and predict accordingly. This prediction will be correct as far as Column 20, but on 21 it will be wrong. After a few more errors he will learn that "brown" is his best prediction, as in fact it is to the end of the row. Chances are good that the subject will assume the second row to be exactly like the first, in which case he will guess it with no errors; otherwise he may make an error or two at the beginning, or at the edge of the "table," as before. He is almost certain to be entirely correct on Row 3, and on subsequent rows through 20. On Row 21, however, it is equally certain that he will erroneously predict a transition from white to brown on Column 21, where the *corner* of the table is passed.

Our subject's behavior to this point demonstrates two principles which may be discussed before we follow him through the remainder of his predictions. It is evident that redundant visual stimulation results from either (*a*) an area of homogeneous color ("color" is used in the broad sense here, and includes brightness), or (*b*) a contour of homogeneous direction or slope. In other words, information is concentrated along contours (i.e., regions where color changes abruptly),[2] and is further concentrated at those points on a contour at which its direction changes most rapidly (i.e., at angles or peaks of curvature).

[2] Our "scanning" procedure introduces a certain artifact here, in that a particular subject will make errors at a linear contour only the first few times he crosses it. It is fairly obvious that if the starting point of the sequence and the direction of scan were varied randomly over a large number of subjects, summated errors would be distributed evenly along such a straight contour.

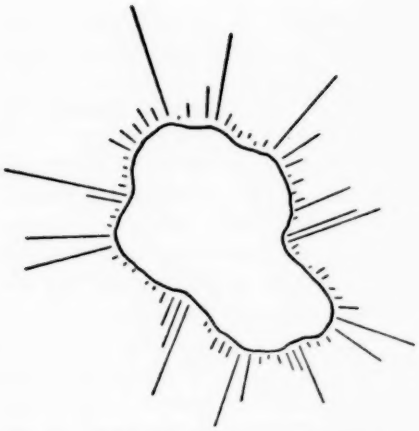

FIG. 2. Subjects attempted to approximate the closed figure shown above with a pattern of 10 dots. Radiating bars indicate the relative frequency with which various portions of the outline were represented by dots chosen.

Evidence from other and entirely different situations supports both of these inferences. The concentration of information in contours is illustrated by the remarkably similar appearance of objects alike in contour and different otherwise. The "same" triangle, for example, may be either white on black or green on white. Even more impressive is the familiar fact that an artist's sketch, in which lines are substituted for sharp color gradients, may constitute a readily identifiable representation of a person or thing.

An experiment relevant to the second principle, i.e., that information is further concentrated at points where a contour changes direction most rapidly, may be summarized briefly.[3] Eighty *S*s were instructed to draw, for each of 16 outline shapes, a pattern of 10 dots which would resemble the shape as closely as possible, and then to indicate on the original outline the exact places which the dots represented. A good sample of the results is shown in Fig. 2: radial bars indicate the relative frequency with which dots were placed on each of the segments into which the contour was divided for scoring purposes. It is clear that *S*s show a great deal of agreement in their abstractions of points best representing the shape, and most of these points are taken from regions where the contour is most different from a straight line. This conclusion is verified by detailed comparisons of dot frequencies with measured curvatures on both the figure shown and others.

Common objects may be represented with great economy, and fairly striking fidelity, by copying the points at which their contours change direction maximally, and then connecting these points appropriately with a straightedge. Figure 3 was drawn by applying this technique, as mechanically as possible, to a real sleeping cat. The informational content of a drawing like this may be considered to consist of two components: one describing the positions of the points, the other indicating which points are connected with which others. The first of these components will almost always contain more information than the second, but its exact share will depend upon the precision with which positions are designated, and will further vary from object to object.

Let us now return to the hypothetical subject whom we left between the corner

FIG. 3. Drawing made by abstracting 38 points of maximum curvature from the contours of a sleeping cat, and connecting these points appropriately with a straightedge.

[3] This study has been previously published only in the form of a mimeographed note: "The Relative Importance of Parts of a Contour," Research Note P&MS 51–8, Human Resources Research Center, November 1951.

of the table and the ink bottle in Fig. 1. His errors will follow the principles we have just been discussing until he reaches the serrated shoulders of the bottle. (A straight 45° line would be represented in this way because of the grain of the coordinate system, but we shall consider that the bottle is actually serrated, as it is from the subject's point of view.) On the left shoulder there are 13 right angles, but these angles contain considerably less than 13 times the information of an angle in isolation like the corner of the table. This is true because they fall into a pattern which is repetitive, or redundant in the everyday sense of the term. They will cease to evoke errors as soon as *S* perceives their regularity and extrapolates it. This extrapolation, precisely like *S*'s previous extrapolations of color and slope, will have validity only over a limited range and will itself lead to error on Row 38, Column 48.

At about the same time that he discovers the regularity of the stair-step pattern (or perhaps a little before), our *S* will also perceive that the ink bottle is symmetrical, i.e., that the right contour is predictable from the left one by means of a simple reversal. As a result he is very unlikely to make any further errors on the right side above Row 32 or 33. Symmetry, then, constitutes another form of redundancy.[4]

It should be fairly evident by now that many of the gestalt principles of perceptual organization pertain essentially to information distribution. The *good gestalt* is a figure with some high degree of internal redundancy. That the grouping laws of *similarity, good continuation,* and *common fate* all refer to conditions which reduce uncertainty is clear enough after the preceding discussion, and we shall presently see that *proximity* may be conceptualized in a like manner. It is not surprising that the perceptual machinery should "group" those portions of its input which share the same information: any system handling redundant information in an efficient manner would necessarily do something of the sort. Musatti (20) came very close to the present point when he suggested that a single principle of *homogeneity* might subsume Wertheimer's laws as special cases. All of our hypothetical *S*'s extrapolations have involved some variety of homogeneity (or invariance), either of color, of slope, or of pattern.

The kinds of extrapolation that have been discussed certainly do not exhaust the repertory of the human observer. For example, if the brightness of a surface were changed at a constant rate along some spatial extent, an observer could probably extrapolate this change with a fair degree of accuracy (given an appropriate response medium, such as choosing from a set of Munsell color patches). Likewise, we may reasonably suppose that a contour, the direction of which changes at a constant rate (i.e., the arc of a circle), could be extrapolated. Any sort of physical invariance whatsoever constitutes a source of redundancy for an organism capable of abstracting the invariance and utilizing it appropriately, but we actually know very little about the limits of the human perceptual machinery with respect to such abilities. A group of psychophysi-

[4] The reader may be comforted to know that six subjects have actually been run on the task described. Their errors, which ranged in number from 13 to 26, were distributed as suggested above, with a single interesting exception: 4 of the 6 *S*s assumed on Row 1 that the brown area would be located symmetrically within the field, and guessed "white" on Column 61. By the use of Shannon's formulas (17) it was estimated that the field contains between 34 (lower limit) and 156 (upper limit) bits of information, in contrast to a possible maximum of 6,340 bits. The redundancy is thus calculated to be between 97.5 and 99.5 per cent.

cal studies determining the accuracy with which observers are able to extrapolate certain discrete and continuous functions of varying complexity must be carried out before we can usefully discuss any but the simplest cases.[5]

A troublesome question arises in this connection: where does perception leave off and inductive reasoning begin? The abstraction of simple homogeneities from a visual field does not appear to be different, in its formal aspects, from the induction of a highly general scientific law from a mass of experimental data. Certain subjective differences are obvious enough: thus reasoning seems to involve conscious effort, whereas perception seems to involve a set of processes whereby information is *predigested* before it ever reaches awareness. When extrapolations are required of a subject in an experimental situation, however, it is difficult or impossible for the experimenter to be certain whether the subject is responding on an "intuitive" or a "deliberative" basis. I do not know any general solution to this problem, and can only suggest that a limited control may be exercised by way of the establishment of a desired set in the subject.

[5] There is, however, a great deal more that can be said about the simplest cases. Vernier acuity demonstrates that, under optimal conditions, error of extrapolation may be less than the "minimum separable." It has been found by Salomon (**16**) that the error made in "aiming" a line at a point some constant distance from its end is a decreasing, negatively accelerated function of the line's length. This may be taken to mean that increasing the length of a line adds information about its extension, but at a decreasing rate, somewhat as increasing the length of a passage of English text adds decreasing increments of information about the next letter (**13, 17**). Dr. Karl Zener, under whose direction the Salomon study was done, is at present conducting a program of related psychophysical experiments which may answer some of the questions raised above.

THE ABSTRACTION OF STATISTICAL PARAMETERS

Although Fig. 1 presents a situation much simpler, or more redundant, than the visual situations which ordinarily confront us, the reader need merely look around the room in which he is sitting to find that the principles illustrated apply to the real world. Further, it may be argued on neurological grounds that the human brain could not possibly utilize all the information provided by states of stimulation which were not highly redundant. According to Polyak's (**14**) estimate, the retina contains not less than four million cones. At any given instant each of these cones may be in either of two states: firing or not firing. Thus the retina as a whole might be in any one of about $2^{4,000,000}$ or $10^{1,200,000}$ states, each representing a different configuration of visual stimulation. Now, if by some unspecified mechanism each of these states were to evoke a different unitary response, and if a unitary response consists merely of the firing of a single unique neuron, then $10^{1,200,000}$ of such response-neurons would be required. The fantastic magnitude of this figure becomes somewhat apparent when one calculates that only about 10^{54} neurons could be packed into a cubic light year. The fact that the number of *patterns* of response-neurons might plausibly equal the number of retinal configurations simplifies matters only if there are certain one-to-one connections between cones and response-neurons, in which case the response is to some degree merely a copy of the stimulus.

We may nevertheless ask: how *would* an observer respond to a situation in which the retinal receptors were stimulated quite independently of one another? This situation would be in practice very difficult to achieve (even more difficult than its diametric opposite, the

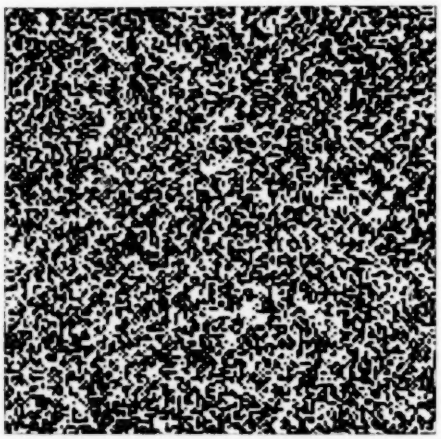

FIG. 4. A "random field" consisting of 19,600 cells. The state of each cell (black vs. white) was determined independently with a p of .50.

Ganzfeld), particularly if we demanded that the stimulation at a given moment (which might be supposed to have a duration of about 100 msec. [see Attneave and McReynolds, 1]) be entirely independent of the stimulation at any other moment. In an effort to get some notion of what such a random field would be like, Fig. 4 was constructed. Each of the $140^2 = 19{,}600$ small cells of the figure was either filled or not filled according to the value of a number obtained from a conversion of Snedecor's (18) table of random numbers from decimal to binary.[6] If the figure is viewed from a distance such that the angle subtended by a cell is of the order of the "minimum separable" (about 1′), it illustrates roughly how a small portion of the random field suggested above might look at some particular instant. Perhaps the most striking thing about the figure is the subjective impression of *homogeneity* that it gives: the left half of the figure seems, at least in a general way, very much like the right half. This is remarkable because we have previously associated homogeneity with redundancy, and Fig. 4 was constructed to be completely nonredundant. Now, in psychological terms, it is fairly clear that the characteristic with respect to which the figure appears homogeneous is what Gibson (6) would call its *texture*. In physical terms, two invariant factors may be specified: (*a*) the probability (.50) that any cell will be black rather than white, and (*b*) the size of cells. Both of these factors probably contribute to perceived texture, which is undoubtedly a multidimensional variable, though the latter may be somewhat the more important. If the figure is viewed from a sufficient distance, these two parameters become identifiable with (*a*) the central tendency, and (*b*) the dispersion, of a continuous brightness distribution in two dimensions.

It appears, then, that when some portion of the visual field contains a quantity of information grossly in excess of the observer's perceptual capacity, he treats those components of information which do not have redundant representation somewhat as a statistician treats "error variance," averaging out particulars and abstracting certain statistical homogeneities. Such an averaging process was involved in drawing the cat for Fig. 3. It was said earlier that the points of the drawing corresponded to places of maximum curvature on the contour of the cat, but this was not strictly correct; if the principle had been followed rigidly, it would have been necessary to represent the ends of individual hairs by points. In observ-

[6] This laborious task was carried out by Airmen 1/C W. H. Price and E. F. Chiburis. Unfortunately, a slight distortion of the relative sizes of black and white cells was introduced in the photographic copying process. The figure was constructed not only for demonstration purposes, but also to serve as a source of random patterns for experimental use. It may also be used wherever a table of random binary numbers is needed, facilitating, for example, the selection of random "draws" from a binomial distribution.

ing a cat, however, one does not ordinarily perceive its hairs as individual entities; instead one perceives that the cat is *furry*. Furriness is a kind of texture; the statistical parameters which characterize it presumably involve averages of shape and direction, as well as size, of elements. The perceived *contour* of a cat (e.g., the contour from which the points of Fig. 3 were taken) is the resultant of an orthogonal averaging process in which texture is eliminated or smoothed out almost entirely, somewhat as if a photograph of the object were blurred and then printed on high-contrast paper (cf. Rashevsky, **15**, and Culbertson, **5**).

The sense in which a surface of a particular texture may be said to provide redundant stimulation has perhaps been adequately indicated. This sort of redundancy might be demonstrated by the guessing-game technique, with a suitable modification in the level of prediction required, i.e., by increasing the unit area to be predicted and requiring the subject to select from a multidimensional array of samples the texture (i.e., the statistical parameters) which he believes the next unit will have. In view of Gibson's (**6**) convincing argument that a physical edge, or contour, is as likely to be represented in vision by an abrupt texture change as by an abrupt color change, I have considered it important to show how texture may be substituted for color without materially altering the principles derived from Fig. 1.

PERCEPTION AS ECONOMICAL DESCRIPTION

It is sometimes said that the objective of science is to describe nature economically. We have reason to believe, however, that some such process of parsimonious description has its beginnings on a fairly naive perceptual level, in scientists and their fellow organisms alike; thus the difficulty, mentioned earlier, of distinguishing between perception and inductive reasoning. It appears likely that a major function of the perceptual machinery is to strip away some of the redundancy of stimulation, to describe or encode incoming information in a form more economical than that in which it impinges on the receptors.

If this point of view is sound, we should be able to generate plausible hypotheses as to the nature of specific perceptual processes by considering rational operations which one might deliberately employ to reduce redundancy. The approach suggested, as it applies to the perception of a static visual field, is equivalent to that of a communications engineer who wishes to design a system for transmitting pictures of real things over a practically noise-free channel with the utmost economy of channel time and band width, but in a manner designed to meet standards such as human observers are likely to have. Some of the reduction principles which he might usefully employ in such a system are listed below. It will be found that these principles serve to summarize and integrate ideas which have been developed somewhat informally in the foregoing sections, as well as to introduce new considerations. The principles may be grouped according to the forms of redundancy with which they are concerned: thus 1–4 deal with varieties of continuous regularity; 5 and 6 with discontinuous regularity, or recurrence; 7–9 with proximity; and 10 with situations involving interaction.

1. An area of homogeneous color may be described by specifying the color and the boundaries of the area over which it is homogeneous. (It is assumed that limits of error tolerance on relevant dimensions have been agreed upon, e.g., that there is some definite number of

colors from which the receiving mechanism may be directed to choose.)

2. Likewise, an area of homogeneous texture may be described by specifying the statistical parameters which characterize the texture and the boundaries of the area over which these parameters are relatively invariant. Thus, if Fig. 4 represented a part of the upholstery of a sofa, it would probably be satisfactory simply to instruct the receiving mechanism to reproduce the texture by filling in cells of a certain size from any table of random numbers. It is true that this process would result in the complete loss of 19,600 bits of information; the essential point is that we are dealing here with a class of stimuli from which such a huge information loss is perceptually tolerable.

3. An area over which either color or texture varies according to some regular function may be described by specifying the function and the boundaries of the area over which it obtains (cf. Gibson's [6] *texture gradient*). This principle actually implies both 1 and 2 as special cases.

4. Likewise, if some segment of an area boundary (i.e., contour) either maintains a constant direction or varies according to some other regular function, it may be described by specifying the function and the loci of its limiting points. Figure 3 illustrates a special case of this principle.

5. If two or more identical stimulus patterns (these might be either successive portions of a contour, or separate and discrete objects) appear at different places in the same field, all may be described by describing one and specifying the positions of the others and the fact that they are identical (cf. *similarity* as a grouping law).

6. If two or more patterns are similar but not identical, it may be economical to proceed as in 5, in addition specifying either (*a*) how subsequent patterns differ from the first, or else (*b*) how each pattern differs from some skeleton pattern which includes the communalities of the group (cf. the "schema-with-correction" idea discussed by Woodworth [20]; also Hebb's [7] treatment of perceptual schemata).

7. When the spatial loci of a number of points are to be described in some arbitrary order, and the points are arranged in clusters or proximity groups (as in Fig. 5), it may be economical to describe the points of each group with respect to some local origin (O' or O''), transmitting as a separate component the positions of the local origins with respect either to each other or to some arbitrary origin, whichever is required. Since the points occupy a smaller range of alternative coordinates on the local axes than on arbitrary axes, less information is required for their specification. If the amount of information thus saved is greater than the amount needed to specify the positions of the local origins, a net saving will result. What is redundant in the present case is the *approximate location* of points in a cluster: this component is isolated out when a local origin is described (cf. the con-

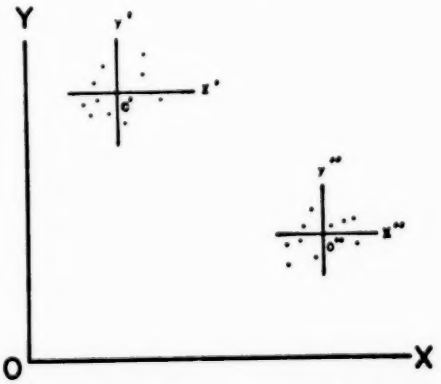

FIG. 5. A functional aspect of proximity-grouping is illustrated. The loci of clustered points may be described with choices from a smaller set of numbers if local origins are used.

cepts of "within" and "between" variance). The local origin principle may also be used in conjunction with some regular scanning procedure if the order in which the points are to be specified is not predetermined (but see also 9, below). The relevance of these considerations to *proximity* as a perceptual grouping law is evident.

8. The preceding principle may be generalized to apply to dimensions other than spatial ones; e.g., brightness, coarseness of texture, etc. A "local origin" on such a continuum would appear to have essentially the characteristics of Helson's (8) *adaptation-level*, in terms of which constancy phenomena and a variety of other psychophysical findings may be accounted for. This generalized principle is closely similar to 6 above, the chief difference being that 6 is applicable to combinations of discrete variables, or to situations of ambiguous dimensional organization.

9. If the loci of a number of points are to be described, and the order in which they are taken is immaterial, they may be arranged in a sequence such that the distances between adjacent points are minimized, and transmitted with each point serving as origin for the one following it. This procedure will result in some saving if the points are clustered, as in Fig. 5, but it is most clearly applicable when the points are "strung-out" in some obvious sequence. In the latter case, a further economy may be achieved by the use of special coordinates such as distance from a line passing through the two preceding points (or from an arc through the three preceding points, etc.; cf. 4 above).

10. Certain areas and objects may be described in a relatively simple way, by procedures of the sort suggested above, if they are first subjected to some systematic distortion or transformation. Consider the case of a complex, symmetrical, two-dimensional pattern viewed from an angle such that its retinal or photographic image is not symmetrical. It will be economical to transmit a description of the pattern as if it were in the frontal plane, and thus symmetrical (eliminating the redundancy of symmetry by means of 6*a*), together with a description of the transformation which relates the frontal aspect described to the oblique aspect in which the pattern is viewed (cf. Gibson's [6] discussion of perspective transformations; also the "Thompsonian coordinates" of D'Arcy Thompson [19]). Koffka (10) and other gestalt psychologists have held that many objects have some "preferred" aspect, and that this aspect has the characteristics of a "good gestalt." The present principle supports this view on functional grounds, since the perceptual transformation of a figure to an aspect in which similarities among parts are maximized may be interpreted as the initial step in an efficient information-digesting process. It should be clearly recognized, however, that an over-all economy is achieved only if the amount of information required to describe the transformation is less than the amount of information saved by virtue of the transformation; thus a transformation must be relatively simple to be considered useful, at least by the present criterion.

Let me indicate briefly how these considerations may be integrated with others of a more general nature. Interdependencies among sensory events may exist either in space or in time, or they may cut across both space and time. In studying the redundancy of spoken English (11), for example, one is dealing with interdependencies which may be considered purely temporal. The present discussion has been restricted, quite arbitrarily, to relationships in space: to forms of redundancy and information-distribution which may obtain in the

visual field at a particular instant, and which a computer of conceivable complexity might evaluate from a photograph. The extension of the visual field in time, which I propose to discuss in a subsequent paper, introduces new varieties of redundancy involving the temporal continuation or recurrence of spatial configurations which may be nonredundant at any instant considered in isolation. Any individual learns a great deal, over his life span, about what-goes-with-what. Thus, if an ear is disclosed in a situation like that illustrated by Fig. 1, the observer can predict that a mouth, nose, eyes, etc. are also present, and approximately where they are. This sort of redundancy is spatiotemporal in its basis; predictions are not possible merely on the basis of the present visual field, but depend also upon previous fields which have contained faces. Principle 6 above suggests the approach to economical description which might be extended to such cases. Further, as Brunswik (2, 3, 4) has pointed out in some detail, ecological principles of very broad generality may be derived from experience.[7] For example, the frequency with which an observer has encountered symmetrical objects in his past may certainly affect the point at which, in predicting successive cells of Fig. 1, he "assumes" that the ink bottle is symmetrical. Likewise in terms of economical encoding: each of the varieties of spatial redundancy suggested above will itself occur with some determinate frequency over any given set of fields (e.g., the set of pictures which a computer-transmitter might have been required to handle over some period of past operation), and a knowledge of this and related frequencies may be used in determining the optimal assignments of actual code symbols. As a result of factors such as these, spatial and spatiotemporal redundancy (or entropy) are difficult to separate empirically, but the distinction remains a conceptually convenient one.

The foregoing reduction principles make no pretense to exhaustiveness. It should be emphasized that there are as many kinds of redundancy in the visual field as there are kinds of regularity or lawfulness; an attempt to consider them all would be somewhat presumptuous on one hand, and almost certainly irrelevant to perceptual processes on the other. It may further be admitted that the principles which have been given are themselves highly redundant in the sense that they could be stated much more economically on a higher level of abstraction. This logical redundancy is not inadvertent, however: if one were faced with the engineering problem suggested earlier, he would undoubtedly find it necessary to break the problem down in some manner such as the foregoing, and to design a multiplicity of mechanisms to perform operations of the sort indicated (some principles, e.g., 6, would require further breakdown for this purpose). Likewise, the principles are frankly intended to suggest operations which the perceptual machinery may actually perform, and accordingly the types of measurement which are likely to prove appropriate in the quantitative psychophysical study of complex perceptual processes.

[7] Brunswik (2), Hebb (7), and the Ames group at Princeton (9) have advanced views concerning the role of experience in perception which have much in common with one another and with my own position in the matter. It appears to me, however, that they have in general tended to underestimate (as the gestalt psychologists have somewhat overestimated) the importance of lawful relationships which may exist within the static and isolated visual field.

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THE NATURE AND MEANING OF DOGMATISM [1]

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In this paper we will attempt to provide the theoretical groundwork for a research project on the phenomenon of dogmatism in various spheres of human activity—political, religious, and scientific. Our main purpose is to present a detailed theoretical statement of the construct of dogmatism which is guiding the research. More specifically, we will define the phenomenon of dogmatism by representing it as a hypothetical cognitive state which mediates objective reality within the person, describe the properties of its organization, and present a number of postulates regarding the relation between dogmatism and other variables. On the basis of part of such a formulation we have thus far developed a preliminary scale for measuring individual differences in dogmatism and several testable hypotheses relevant to our conceptual definition (to be reported subsequently).

A second purpose stems from the fact that our construct of dogmatism involves the convergence of three highly interrelated sets of variables: closed cognitive systems, authoritarianism, and intolerance. By virtue of this convergence it will be possible to examine certain assumptions underlying previous research on authoritarianism and intolerance (1) with the aim of achieving a possibly broader conceptualization of these phenomena.

It is not within the scope of this paper to inquire into the social or personal conditions which give rise to dogmatism. This is considered to be an independent theoretical problem. Having defined the problem of dogmatism and its representation at the cognitive level—the main purpose of this paper—one can then seek explanations according to one's theoretical orientation.

A basic assumption guiding the present formulation is that despite differences in ideological content, analysis will reveal certain uniformities in the structure, the function, and, to some extent, even the content of dogmatism. Accordingly, attention will be directed to both political and religious dogmatism and, within each area, to diverse and even opposed dogmatic orientations. In the religious sphere, for example, one can observe expressions of dogmatic Catholicism and dogmatic anti-Catholicism, dogmatic orthodox Judaism and dogmatic antiorthodox Judaism, dogmatic theism and dogmatic atheism. In the political sphere one can observe expressions of dogmatic conservatism and dogmatic liberalism, dogmatic Marxism and dogmatic anti-Marxism.[2]

The problem of dogmatism, however, is not necessarily restricted to the political and religious spheres. It can be observed in other realms of intellectual and cultural activity—in philosophy, the humanities, and the social sciences. To take some examples from psychology, it is possible to observe expressions of dog-

[1] Written while the writer was a Social Science Research Council Faculty Research Fellow.

[2] In late 1951 we came across Eric Hoffer's *The True Believer* (4). We have profited greatly from this book, which provides many insights into the nature of mass movements and into the "true believer" who leads such movements. We wish to acknowledge our intellectual debt to Hoffer for a number of ideas to be presented here and in future papers which owe their origin to him.

matic Freudianism and dogmatic anti-Freudianism, dogmatic learning theory and dogmatic antilearning theory, dogmatic gestalt theory and dogmatic anti-gestalt theory, and so forth.

Dogmatism, furthermore, need not necessarily involve adherence to this or that group-shared, institutionalized system of beliefs. It is conceivable that a person, especially one in academic circles, can be dogmatic in his own idiosyncratic way, evolving a unique rather than institutionalized integration of ideas and beliefs about reality. The present formulation will attempt to address itself to noninstitutional as well as institutional aspects of dogmatism.

General Setting for a Cognitive Representation of Dogmatism

To conceptualize dogmatism adequately at the cognitive level it is first necessary to employ a set of conceptual tools in terms of which all cognitive systems, varying in degree of dogmatism, may be represented.

Organization into Belief and Disbelief Systems

Objective reality can be assumed as being represented within a person by certain beliefs or expectations which to one degree or another are accepted as true, and other beliefs or expectations accepted as false. For the sake of analysis this can be formalized by conceiving of all cognitive systems as being organized into two interdependent parts: a belief system and a disbelief system. This belief-disbelief system can further be conceived as varying in terms of its structure and content as follows:

Structure. The total structure of a belief-disbelief system can be described as varying along a continuum from open to closed. This continuum, in turn, may be conceived as a joint function of: (*a*) The degree of interdependence among the parts within the belief system, within the disbelief system, and between belief and disbelief systems (**6**, **7**, **9**, **12**). (*b*) The degree of interdependence between central and peripheral regions of the belief-disbelief system (**9**). (*c*) The organization of the belief-disbelief system along the time perspective dimension (**2**, **3**, **4**, **8**).

Content. One can further describe all belief-disbelief systems in terms of the formal content of centrally located beliefs, especially those having to do with beliefs about authority and people in general.

A Cognitive Representation of Dogmatism

In line with the above considerations we will now define dogmatism as (*a*) a relatively closed cognitive organization of beliefs and disbeliefs about reality, (*b*) organized around a central set of beliefs about absolute authority which, in turn, (*c*) provides a framework for patterns of intolerance and qualified tolerance toward others. A cognitive organization is considered to be closed to the extent that there is (*a*) isolation of parts within the belief system and between belief and disbelief systems, (*b*) a discrepancy in the degree of differentiation between belief and disbelief systems, (*c*) dedifferentiation within the disbelief system, (*d*) a high degree of interdependence between central and peripheral beliefs, (*e*) a low degree of interdependence among peripheral beliefs, and (*f*) a narrowing of the time perspective.

More specifically, in the relatively closed belief-disbelief system there is assumed to be a relation of relative isolation among the various parts of the belief system and between belief and disbelief systems. The latter, in turn, is composed of a series of disbelief sub-

systems, each arranged along a gradient of similarity to the belief system, the most similar disbelief subsystems being represented as regions most adjacent to the belief system. Each of these disbelief subsystems is conceived, to the extent that it is part of a closed system, as being relatively less differentiated than the belief system and, the farther away their positions from the belief system, as increasingly dedifferentiated with respect to each other.

Belief-disbelief systems can also be represented along a central-peripheral dimension. The more closed the system the more the central part corresponds to absolute beliefs in or about authority, and the more the peripheral part corresponds to beliefs and disbeliefs perceived to emanate from such authority.

With respect to the time perspective dimension, increasingly closed systems can be conceived as being organized in a relatively future-oriented or past-oriented direction rather than in terms of a more balanced orientation of past, present, and future.

With regard to the content of dogmatism, while the specific content of both central and peripheral parts may vary from one particular ideological system to another, it is possible to specify that in general the formal content of the central part of the system, to the extent it is closed, has to do with absolute beliefs in and about positive and negative authority, either external or internal, and related beliefs representing attempts on the part of such authority to perpetuate itself. Furthermore, the central part can be conceived to provide a framework for the organization of other beliefs representing patterns of rejection and qualified acceptance of people in general according to their patterns of agreement and disagreement with the belief-disbelief system.

DOGMATISM DISTINGUISHED FROM RIGIDITY

Before going on to elaborate further on our conceptual definition of dogmatism, it may be illuminating first to distinguish the construct of dogmatism in a general way from that of rigidity. Both dogmatism and rigidity refer to forms of resistance to change, but dogmatism is conceived to represent a relatively more intellectualized and abstract form than rigidity. Whereas dogmatism refers to total cognitive *organizations* of ideas and beliefs into relatively closed ideological systems, rigidity, when genotypically conceived, refers solely to the degree of isolation between regions (**7**, **12**) or to a "property of a functional boundary which prevents communication between neighboring regions" (**6**, p. 157); when phenotypically conceived, rigidity is defined in terms of the way a person or animal attacks, solves, or learns *specific* tasks and problems (**11**). Thus, dogmatism is seen as a higher-order and more complexly organized form of resistance to change. While dogmatism may well be hypothesized to lead to rigidity in solving specific problems, the converse is not necessarily the case. Rats, the feebleminded, and the brain-injured, for example, can be characterized as rigid (also compulsive, fixated, perseverative, inflexible) but hardly as dogmatic.

Furthermore, whereas rigidity refers to person-to-thing or animal-to-thing relationships, dogmatism is manifested almost necessarily in situations involving person-to-person communication. Thus, we speak of a person as tying his shoelaces or solving an arithmetic problem rigidly, but of a professor, a politician, an orator, a theoretician, or an art critic as expressing himself to others dogmatically.

A final differentiation, closely related to the preceding, is that dogmatism has

a further reference to the authoritarian and intolerant manner in which ideas and beliefs are communicated to others. Thus, the range of behavior considered under the rubric of dogmatism is considerably broader than rigidity and at the same time of possibly more intrinsic interest to the sociologist, the political scientist, and the historian, as well as the psychologist.[3]

POSTULATES INVOLVING THE COGNITIVE STRUCTURE OF DOGMATISM

To the extent that the belief-disbelief system is closed, it is subjected to continual stresses and strains from objective and social reality. Reality can be coerced into congruence with the belief-disbelief system by virtue of the arrangement of parts within the belief-disbelief systems, within and between the central and peripheral regions thereof, and by virtue of its organization along the time perspective dimension.

A. Isolation within and between Belief and Disbelief Systems

The greater the dogmatism the greater are the assumed degree of isolation or independence between the belief and disbelief systems and the assumed degree of isolation among the various parts of the belief system. On the basis of these considerations we introduce the following postulates:

1. *Accentuation of differences between belief and disbelief systems.* The greater the dogmatism the more will the belief system be perceived as different in content or aim from the disbelief system (e.g., Catholicism and Protestantism; the United States and the USSR; fascism and communism; psychoanalysis and behaviorism).

2. *The perception of irrelevance.* The greater the dogmatism the more will ideological arguments pointing to similarities between belief and disbelief systems be perceived as irrelevant.

3. *Denial.* The greater the dogmatism the greater the denial of events contradicting or threatening one's belief system (e.g., on grounds of "face absurdity," that the true facts are not accessible, that the only available sources of information are biased because they are seen to emanate from the disbelief system, etc.).

4. *Coexistence of contradictions within the belief system.* In line with the assumption that in increasingly closed cognitive organizations there is relatively more isolation among subparts of the belief system, as well as between belief and disbelief systems, it is postulated that the degree of adherence to contradictory beliefs will vary directly with the degree of dogmatism. Some examples of contradictory beliefs are an abhorrence of violence together with the belief that it is justifiable under certain conditions; expressions of faith in the intelligence of the common man and at the same time the belief that the masses are stupid; a belief in democracy and along with this the belief that our country can best be run by an intellectual elite; a belief in freedom for all but at the same time the belief that freedom for certain groups should be restricted; a belief that science makes no value judgments about "good" and "bad" but also that scientific criteria are available for distinguishing "good" theory from "bad" theory, and "good" experiment from "bad" experiment (13).

[3] We may mention in passing a third and conceivably higher level of resistance to change —intellectual conviction. This refers to rational rather than rationalized beliefs rooted in demonstrably closer congruence with objective reality and, thus, organized into more open and comprehensive cognitive organizations (12). A preliminary measure of intellectual conviction has been devised in collaboration with Dr. Albert Eglash and will be reported in a future publication.

B. The Disbelief Gradient

We have already indicated that in relatively closed systems there is relative isolation between belief and disbelief systems. However, the various disbelief subsystems cannot all be assumed to be equally isolated from the belief system. Rather, degree of isolation can further be conceived as varying with the degree of perceived similarity of the various disbelief subsystems to the belief system. Those disbelief subsystems most similar to the belief system can be represented as regions most adjacent to the belief region and, hence, in relatively greater communication or interaction with the belief system than less similar disbelief subsystems.

1. *Strength of rejection of various disbelief subsystems.* The greater the dogmatism the more will the disbelief subsystem most similar to the belief system (factional or "renegade" subsystems) be perceived as threatening the validity of the belief system and hence the greater the tendency to exert effort designed to reject this subsystem and the adherents thereof. For example, with an increase in dogmatism there will be an increasingly militant rejection of Protestantism by the Catholic, of reformed Judaism by the more conservative Jew, of Trotskyism and Titoism by the Communist, and vice versa. In the academic realm, too, the greater the dogmatism the more antagonism there will be among representatives of divergent views within a single discipline as compared with related disciplines.

2. *Willingness to compromise.* Even though a person or group may reject a disbelief system it is often necessary, for the sake of achieving political or religious aims, to form working alliances with other individuals or groups. It is here postulated that such compromising varies inversely with dogmatism: the greater the dogmatism the less compromise there will be with adherents to the disbelief subsystem closest to the belief system.

C. Relative Degrees of Differentiation of Belief and Disbelief Systems

In our cognitive representation of dogmatism we have assumed that the greater the dogmatism the more differentiated the belief system will be as compared with the disbelief system. Moreover, various disbelief subsystems have been assumed to become relatively more dedifferentiated with respect to each other the farther away their positions from the belief system. The following postulates are based upon these considerations:

1. *Relative amount of knowledge possessed.* The greater the dogmatism the greater the discrepancy between degree of knowledge of facts, events, ideas, and interpretations stemming from the belief system and any one of the disbelief subsystems. Thus, for example, with an increase in dogmatism there will be an increasing discrepancy in the Freudian's knowledge of classical psychoanalysis as compared with Adlerian psychology or learning theory.

2. *Juxtaposition of beliefs and disbeliefs.* Under special conditions, however, e.g., social or personal conditions which lead to disillusionment with the belief system and thence to conversion wherein beliefs and disbeliefs become juxtaposed, it is conceivable that one of the disbelief subsystems will be more differentiated than the belief system. It is therefore postulated that as a function of the reversal of belief and disbelief systems, the greater the dogmatism the greater the discrepancy between degrees of differentiation of belief and disbelief systems in favor of the latter.

3. *Dedifferentiation within the disbelief system.* The greater the dogmatism the more will two or more disbelief sub-

systems represented as positions relatively far away from the belief system along the disbelief gradient be perceived as "the same" (e.g., that communism and socialism are the same, that the Democrats and Republicans are both run by Wall Street, etc.).

D. Relation between Central and Peripheral Parts

We have assumed further that, to the extent we are dealing with closed systems, the central part corresponds to beliefs in and about absolute authority and the peripheral part to beliefs and disbeliefs perceived to emanate from such authority. Thus, the more closed the system the greater the assumed degree of communication between central and peripheral beliefs and, at the same time, the less the assumed degree of communication among the various peripheral beliefs. From these considerations it follows also that any given change in the peripheral part represents an isolated change in cognitive content without concomitant changes either in cognitive structure or in *over-all* ideological content.

It is this interrelation between central and peripheral parts which gives the relatively closed system its integrated and systematic character.[4] Specific peripheral beliefs and disbeliefs are organized together not so much by intrinsic logical connections as by virtue of their perceived origination with positive and negative authority, respectively. Whatever characterizes the authority's ideology, as represented by the central part, will be mirrored "gratuitously" in the closed system. If the authority is logical, the closed system will appear logical; if the authority is illogical, the closed system will appear illogical. The more closed the system the more will it reflect *in toto* the authority's own system with its logic or illogic, its *manifest* intellectualism or anti-intellectualism, and so forth.

1. *"Party-line" changes.* It is commonly observed that relatively dogmatic views on specific issues are stubbornly resistant to change by logical argument or objective evidence. One possible reason for this becomes apparent in the light of the preceding considerations. The greater the dogmatism the more will there be a change in a given peripheral belief (e.g., about birth control) if it is preceded by a perceived corresponding change by the authority (e.g., the Catholic Church). Moreover, the greater the dogmatism the less will any given change in a peripheral belief effect changes in other peripheral beliefs (e.g., about divorce, federal aid to education, etc.).

2. *Assimilation.* Further considerations regarding the relation between central and peripheral parts lead also to the following postulate: The greater the dogmatism the greater the assimilation of facts or events at variance with either the belief or disbelief system by altering or reinterpreting them such that they will no longer be perceived as contradictory.

3. *Narrowing.* Assuming, as we have, that the central region is crucial in determining what aspects of reality will be represented within the peripheral region, it follows that it will also be crucial in determining what aspects of reality will *not* be represented (12). The greater the dogmatism the more the avoidance of contact with stimuli—peo-

[4] This is illustrated very nicely by the following, which we believe is from Sholom Aleichem: "I did not borrow your pot; besides it was broken when you lent it to me; besides I have already returned it to you." Despite its illogical character, the statement is nevertheless systematic. While each of the beliefs expressed is contradictory to the others, they all reinforce each other to serve the end of protecting the central authority (in this case, the person speaking) against threat.

ple, events, books, etc.—which threaten the validity of the belief system or which proselyte for competing disbelief systems.

Cognitive narrowing may be manifested at both institutional and noninstitutional levels. At the institutional level, narrowing may be manifested by the publication of lists of taboo books, the removal and burning of dangerous books, the elimination of those regarded as ideological enemies, the omission of news reports in the mass media unfavorable to the belief system or favorable to the disbelief system, and the conscious and unconscious rewriting of history (**10**).

At the noninstitutional level, narrowing may become apparent from the systematic restriction of one's activities in order to avoid contact with people, books, ideas, social and political events, and other social stimuli which would weaken one's belief system or strengthen part of one's disbelief system. Relevant here are such things as exposing oneself only to one point of view in the press, selectively choosing one's friends and associates solely or primarily on the basis of compatibility of belief systems, selectively avoiding social contact with those adhering to different belief systems, and avoiding those who formerly believed as one does.

In academic circles cognitive narrowing over and above that demanded by present day specialization may be manifested by a selective association with one's colleagues and selective subscription, purchase, and reading of journals and books such that one's belief system becomes increasingly differentiated while one's disbelief system becomes increasingly dedifferentiated or "narrowed out."

E. Time Perspective

1. *Attitude toward the present.* The greater the dogmatism the more will the present be perceived as relatively unimportant in its own right—as but a passageway to some future utopia. Furthermore, with an increase in dogmatism there will be a concomitant increase in the perception of the present as unjust and as full of human suffering.

2. *Belief in force.* Such a disaffected conception of the present can readily lead to the belief that a drastic revision of the present is necessary. Thus, we are also led to the following postulate: The greater the dogmatism the greater the condonement of force.

3. *Knowing the future.* Another aspect of time perspective has to do with one's understanding of the future.[5] With an increase in dogmatism there will be the following variations: an increasing confidence in the accuracy of one's understanding of the future, a generally greater readiness to make predictions, and a decreasing confidence in the predictions of the future made by those adhering to disbelief systems.

POSTULATES INVOLVING THE COGNITIVE CONTENT OF DOGMATISM

We have already pointed out that while the specific content of beliefs and disbeliefs varies from one system to another, it is nevertheless possible to point to certain uniformities in the formal content of centrally located beliefs which, to the extent that they are part of a closed system, form the cognitive bases for authoritarianism and intolerance.

A. Authoritarianism

At the center of the belief-disbelief system, to the extent it is closed, is assumed a set of absolute beliefs about positive and negative authority and other closely related beliefs representing

[5] In "scientific" dogmatism the future may be conceived as referring to research outcomes rather than to the historical future.

attempts by such authority to reinforce and perpetuate itself.

1. *Positive and negative authority.* With an increase in dogmatism there will be not only increasing admiration or glorification of those perceived in positions of positive authority but also increasing fear, hatred, and vilification of those perceived in positions of authority opposed to positive authority.

2. *The cause.* With an increase in dogmatism there will be an increasing strength of belief in a single cause and concomitantly a decreasing tendency to admit the legitimacy of other causes. Manifestations of strength of belief in a single cause might be making verbal references to "the cause," expressing oneself as "feeling sorry" for those who do not believe as one does, believing that one should not compromise with one's ideological enemies, perceiving compromise as synonymous with appeasement, believing that one must be constantly on guard against subversion from within or without, and believing that it is better to die fighting than to submit.

3. *The elite.* With an increase in dogmatism there will be an increase in strength of belief in an elite (political, hereditary, religious, or intellectual).

B. Intolerance

Beliefs in positive and negative authority, the elite, and the cause all have to do with authority as such. Coordinated with such beliefs are others representing organizations of people in general according to the authorities they line up with. In this connection there may be conceived to emerge, with increasing dogmatism, increasingly polarized cognitive distinctions between the faithful and unfaithful, orthodoxy and heresy, loyalty and subversion, Americanism and un-Americanism, and friend and enemy. Those who disagree are to be rejected since they are enemies of God, country, man, the working class, science, or art. Those who agree are to be accepted but only as long as and on condition that they continue to do so. This sort of qualified tolerance is, to our mind, only another form of intolerance. That it can turn quickly into a frank intolerance is often seen in the especially harsh attitude taken toward the renegade from the cause.[6]

It is in this way that the problem of acceptance and rejection of people can become linked not only with authoritarianism but also with the acceptance and rejection of ideas. Perhaps the most clear-cut single behavioral manifestation of this linkage is the employment of opinionated language in communicating beliefs and disbeliefs to others. Opinionation[7] is a double-barrelled sort of variable which refers to verbal communications involving acceptance or rejection of beliefs in an absolute manner and, at the same time, acceptance or rejection of others according to whether they agree or disagree with one's beliefs.

1. *Opinionated rejection.* This refers to verbal statements which imply absolute rejection of a belief and at the same time rejection of persons who accept it. The following examples illustrate this: "Only a simple-minded fool would think that . . . ," "A person must be pretty stupid to think . . . ," "The idea that . . . is pure hogwash (or poppycock, nonsense, silly, preposterous, absurd, crazy, insane, ridiculous, piddling, etc.)."

The preceding considerations lead us to postulate that opinionated rejection will vary directly with dogmatism.

2. *Opinionated acceptance.* This refers to an absolute acceptance of a be-

[6] We have already discussed the concept of a gradient of disbelief systems. In the present connection, we might also point to parallel gradients of intolerance and authority.

[7] In a future paper we will report on a scale designed to measure opinionation.

lief and along with this a qualified acceptance of those who agree with it. Some examples are: "Any intelligent person knows that . . . ," "Plain common sense tells you that . . . ."

Opinionated acceptance will also vary directly with dogmatism.

SOME IMPLICATIONS FOR FURTHER THEORY AND RESEARCH ON AUTHORITARIANISM AND INTOLERANCE

Through the pioneering research of Adorno *et al.* (1) significant theoretical and empirical advances have been made recently in understanding the phenomena of authoritarianism and intolerance. Since our construct of dogmatism also involves a representation of these phenomena, it is proper to ask: To what extent is the present formulation of the problem of dogmatism consonant with the work on the authoritarian personality?

To be noted first is a historical fact. The research on the authoritarian personality was launched at a time when the problem of fascism and its attendant anti-Semitism and ethnocentrism was of overriding concern to both social scientist and layman. Given this social setting as a point of departure, it was almost inevitable that the general problem of authoritarianism would become more or less equated with the problems of adherence to fascist ideology and ethnic intolerance. Thus, the personality scale designed to tap underlying predispositions toward authoritarianism was called the F (for fascism) Scale and was found to correlate substantially with measures of ethnic intolerance.

It is widely recognized, however, that authoritarianism is also manifest among radicals, liberals, and middle-of-the-roaders as well as among conservatives and reactionaries. Furthermore, authoritarianism can be recognized as a problem in such areas as science, art, literature, and philosophy, where fascism and ethnocentrism are not necessarily the main issues or may even be totally absent as issues. As pointed out in this paper, dogmatism, which is assumed to involve both authoritarianism and intolerance, need not necessarily take the form of fascist authoritarianism or ethnic intolerance.

It is thus seen that the total range of phenomena which may properly be regarded as indicative of authoritarianism is considerably broader than that facet of authoritarianism studied so intensively by the authors of *The Authoritarian Personality*. On theoretical grounds, we are in accord with the view that authoritarianism has a greater affinity to leanings to ideologies which are antidemocratic in content. But it need not be conceived as *uniquely* connected with such ideologies. If a theory of authoritarianism is to be a general one, it should also be capable of addressing itself to the fact that to a great extent authoritarianism cuts across specific ideological orientations. As we have tried to suggest, dogmatic authoritarianism may well be observed within the context of any ideological orientation, and in areas of human endeavor relatively removed from the political or religious arena.

One way to test the validity of the above considerations is to demonstrate that scores on the F Scale are related substantially to measures of dogmatism independently of liberalism-conservatism, or of the kinds of attitudes held toward such groups as Jews and Negroes. The results of one study, already reported (14), show that dogmatism and authoritarianism correlate over .60 when ethnocentrism or political-economic conservatism is held constant. Corroborative findings from sev-

eral studies will be presented in a more detailed future report.

Consider further the way the problem of intolerance has been conceived in social-psychological research (**1**, **5**). Parallel to the more or less rough equating of authoritarianism with fascism, and perhaps for similar reasons, intolerance too can be said to have become more or less equated with one aspect of intolerance, namely, ethnic intolerance. Examination reveals that such concepts as intolerance, discrimination, bigotry, social distance, prejudice, race attitudes, and ethnocentrism are all defined operationally in much the same way—by determining how subjects feel or act toward Jews, Negroes, foreigners, and the like.

It is reasonable to assume that there are persons who, although they would validly score low on measures of ethnocentrism or similar scales presently in use, would nevertheless be characteristically intolerant of those whose belief-disbelief systems are at odds with their own.

It has already been suggested that while dogmatic authoritarianism may "attach" itself to any ideology, it is probably more closely related to those having antidemocratic content. A similar point may well be made in connection with dogmatic intolerance. Preliminary data already available suggest that measures of dogmatic intolerance (opinionation) and ethnic intolerance are, as expected, related to each other to a significant degree. At the same time they are also found to cut across each other in a relatively independent fashion.

The preceding considerations point to other aspects of man's intolerance to man, in addition to ethnic intolerance, which deserve scientific attention. And, as we have tried to point out in discussing the problem of authoritarianism, here too we think there is a need for a more comprehensive conceptualization of the problem of intolerance.

SUMMARY

To provide a framework for empirical research we have attempted a conceptual representation of the phenomenon of dogmatism by describing in some detail the properties of its cognitive organization. Dogmatism has been defined as (*a*) a relatively closed cognitive system of beliefs and disbeliefs about reality, (*b*) organized around a central set of beliefs about absolute authority which, in turn, (*c*) provides a framework for patterns of intolerance and qualified tolerance toward others.

We have described this relatively closed cognitive organization in terms of the degree of interdependence among the various parts of the belief-disbelief system, its organization along a central-peripheral dimension, and its organization along a time perspective dimension. We have also described how relatively closed systems could be conceived as being organized around a central part, the formal content of which forms the cognitive bases for patterns of beliefs involving authoritarianism and intolerance.

On the basis of various aspects of this hypothetical model of a continuum ranging from the "closed mind" to the "open mind" we have advanced a series of postulates regarding the relation between dogmatism, conceived as a hypothetical construct having the status of an independent variable, and other variables.

Furthermore, from our theoretical formulation certain implications were drawn for present thinking and research on the problems of authoritarianism and intolerance. We suggested that selective factors may have operated in such a way that research on both authoritarianism and intolerance has be-

come more or less reduced to and synonymous with but one form of authoritarianism, fascism, and with but one form of intolerance, ethnic intolerance. We suggested too that present thinking and research along these lines are in need of extension in the light of both theoretical considerations and findings which indicate that dogmatic authoritarianism and intolerance cut across in an independent fashion such variables as ethnocentrism, political-economic conservatism, and fascist authoritarianism.

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HOMEOSTASIS: CORRUPTIONS OR MISCONCEPTIONS?—A REPLY

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The progress of science depends upon the identification of significant similarities and differences among phenomena. Newton's law of the mutual attraction of masses could not be formulated until the basic similarity of certain superficially different events in nature had been observed. The task facing psychologists is to locate such fundamental similarities—and, of course, to test them by hunting for crucial negative instances. Such uniformities will lead to scientific progress in our field.

This logic dictated the venture, published in 1951 (6), into "unifying" various observations under the rubric of homeostasis. It seemed to me that it was fruitful to speculate about some similarities among equilibrium phenomena at the physiological and the personality levels. The article was intended as an exploratory study, rather than as the full-blown formulation of a theory, which seems implied in the criticisms recently offered by Maze (2). I wish we were in a position to offer detailed hypotheses covering the points he raises.[1] However, I think that some of his criticisms represent misconceptions rather than "corruptions" of the homeostatic point of view.

Let us first consider the question of teleology. Maze states that "the notion arises that organisms possess a special kind of causality; namely, a teleological causality, so that the part-processes within an organism are determined not by the *nature* of those parts . . . but by the effect which *is to be produced* by them in the organism as a whole" (2, p. 407). If this position were adopted, it would indeed be a corruption of the homeostatic view of motivation. But, in fact, both in the 1951 article (6) and in the Stagner-Karwoski text (7), the importance of the part-processes was asserted. The analysis of motivated behavior calls for a determination of which steady state is involved, what threat disturbs the equilibrium, what action is taken, and so on (7, esp. pp. 17, 68, 90, 110–116). Selye (5) and others have shown that the widespread organismic effects following disturbance of equilibria need not lead to a teleological interpretation. However, it is probable that survival of organisms responding vigorously to threats has led to the selection of species showing such apparent relationships (7, p. 112).

Another misconception which Maze might have avoided is his idea that we have interpreted homeostasis as "a vaguely conceived and therefore very accommodating force named 'homeostasis'" (2, p. 406). We have asserted that "it is not correct to speak of the organism as trying to maintain homeostasis, any more than a falling body is trying to maintain gravitation. Falling bodies illustrate the law of gravitation, and specific bodily activities illustrate the principle of homeostasis" (7, p. 17,

[1] Parenthetically, let me note that I consider myself neither the "chief exponent" nor "the leader" in working out the kind of approach to psychological problems which Maze is attacking. However, I enjoy the company in which I find myself, and hope to contribute, along with others, to a solution of some of these vexing problems. But this answer to Maze is in my own behalf and does not commit anyone else to the views presented.

footnote). We therefore have no quarrel with the suggestion Maze offers that psychologists must search for the specific dynamics underlying favorable constancies. Indeed, I considered such a proposal to be implicit in my 1951 article, which suggested that personalities could be studied in terms of the particular equilibria established, the situations perceived as threatening, etc. Recently Menninger (3) has published an approach to the diagnosis of psychiatric cases which is specifically built around the doctrines of homeostasis, favorable equilibria, and energy mobilization to protect these equilibria. The problem of diagnosis, as he sees it, is to identify the particular equilibrium (or equilibria) which the patient is trying to maintain, the stimuli perceived as threatening disequilibration, and the defenses developed by the patient to protect his achieved constancies. Newcomb (4) has likewise proposed that we treat social communication in terms of "strains" toward preferred states of equilibrium. I would certainly agree with Maze that there are many constancies, a disturbance in any one of which may result in energy mobilization (cf. 7, ch. 2, 3, and 4). This is an implication for research based on homeostatic theory.

On the other hand, Maze seems to be proposing an untenable atomism by his emphasis on the variety of motivating states. For example, "What is really happening is that two impulses of approximately equal strength have come into conflict, and each is enlisting still further impulses on its side" (2, p. 411). This picture of little impulses lining up and choosing sides is not only extremely animistic (a viewpoint which Maze imputes to me), but it completely ignores the integrating role of the organism—or, if Maze prefers, the cerebral cortex. When a man is faced with a choice between two impulses, he examines the situation, looks for significant cues, tries to identify short-run and long-run rewards and punishments likely to accrue from specified courses of action, and makes a decision based on these various items of information. In some individuals, specifically those we call psychopaths, this integrative function seems rather inadequate, but we need a theory of motivation covering a much wider range than this limited psychiatric category.

It is at this point that the phenomenological approach to perceived reality becomes a natural ally of homeostatic thinking about motivation. When I get hungry, I get hungry as a total organism (Maze on part-processes to the contrary notwithstanding). If I do not find food, it is the total organism that will die, not just my stomach.[2] The perception of goal objects thus becomes an essential part of the process of maintaining the essential constancy of food-energy balance. It seems to me, as it has to many psychologists, that motivated human behavior has far more the characteristic of an active searching for desired objects (positive valences) than the characteristic of a blind reaction to struggling impulses. How can we relate this conception of motivation to a physiological base? The doctrine of homeostasis seems to give the best linkage available at this time (cf. 7, pp. 64–68).

This phenomenological approach to perception seems to disturb Maze. He adopts what he calls "a realist position," which, he says, is "that the young organism is developing its ability to recognize, under varying conditions, properties which the object *actually does*

[2] The extent to which disturbances of one part-process spread to other part-processes, and in short order, can only be described by referring them to the "total organism," is well illustrated by the work of Selye (5) on the general adaptation syndrome. (See also 7, pp. 112–113.)

possess" (2, p. 409, italics mine). I hope that Maze will some day work through that old paradox which all students in introductory psychology suffer with: Is the grass really green? Is green *actually* a property of grass? Is being six feet tall *actually* a property of Johnny's father? Is the desk *actually* rectangular? Of course not. *Under certain conditions* grass looks green, Johnny's father looks six feet tall, etc.[3] The child gets mutual confirmation from various combinations of visual, auditory, tactual, kinesthetic, and other sensory cues. He also gets social confirmation from the verbal responses of people around him. The immediate percept which guides behavior is the summed probability, the "best bet" the individual can make at this moment, as to what is "really" out there. Under certain conditions, as in the ingenious demonstrations devised by Ames (1), percepts can be induced which are quite contrary to "reality." And, if the individual is prevented from getting certain kinds of cues, he not only accepts the percept as real—he also acts on it. Maze seems to feel that people should not do this, that they should act on the *correct* perception, not the *constant* perception (2, p. 409). But the whole vast literature on perception shows that people act on size constancy, shape constancy, etc., even when these percepts are not at all correct in terms of reality.

Finally, Maze takes exception to my extensions of perceptual constancy into the field of perception of persons. A detailed analysis of this problem is beyond the space of this note; it has already been worked out elsewhere (7, pp. 235–237) in a preliminary form. What Maze seems to ignore is my emphasis on the fact that perceived constancies (of persons) persist long after the "real" person has changed. He also ignores the considerable literature showing that frustration and threat emanating from a specific person can bring about marked distortions in the perception of that person.

The problem of the perceived self is in this same category. The young child gets numerous visual, auditory, somesthetic, and other sensory experiences cued to his own body. The repeated contiguity, in space and time, of these experiences provides a perfect nucleus for an integrated percept. Adults aid in the development of this self-image by providing him with a name, by imputing various characteristics to him which he cannot sense directly, and by rewarding or punishing acts cued to this percept. For example, a perception of the self as inadequate to deal with the environment is easily developed by overcritical parents. The child will then behave as if he were inadequate, even though "in reality" he has the essential capacities.

The extent to which physiological equilibria and psychological "constant states" are interrelated may again be illustrated by the study of Strother and Cook (8). They have shown that the Schneider index, a measure of neurocirculatory efficiency in restoring equilibrium, is significantly correlated with a measure of behavioral disturbance resulting from a perceived frustrating situation. Naturally, we shall want to know what part-processes are involved in this relationship; but the finding as it stands offers some confirmation of the usefulness of the "homeostatic" approach.

[3] In the world of physics, grass, desk, and man are masses of electrons, protons, etc. In the world of chemistry, they are collections of molecules. By mutual agreement we can specify what we will call "reality," but this is a sophisticated judgment far removed from everyday experience.

I am ready to concede that the homeostatic framework needs a great deal more data and more conceptual analysis before it provides the general theory of motivation so urgently needed in psychology. We need to study the thresholds for disequilibrium, factors inducing dominance of one equilibrium over others, modes of protecting equilibria, etc. But at least the theory does not lead to atomistic and animistic formulations such as that offered by Maze: "a given impulse X comes to believe, or is, as it were, assured by impulse Y, that the 'satisfaction' (release of tension) of X is facilitated by the satisfaction of Y and opposed by the satisfaction of Y's opponent, Z" (2, p. 411). Faced with such a proposition, I can only say that, to me, homeostasis looks like a conception of dynamics far more compatible with scientific logic and empirical research.

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